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RECALL AS AN ABSTRACTIVE PROCESS ¹

BY

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1. INTRODUCTION

Long after psychology had begun to become an experimental discipline, the orthodox view of memory remained in essence the one held by the ancient Greeks (3). An experience was thought to leave in the organism a lasting facsimile "trace," organized solely in terms of spatial and temporal relationships inherent in the original experience. Such a trace could be left to fade away, or be preserved and strengthened by repeating or recalling the experience. In recall, appropriate stimuli reactivated the trace, and whatever remained of it at the time was mechanically reproduced. In both experience and recall, the individual's rôle was essentially passive. The possibility that traces might embody an interpretative reaction to experience, or, once formed, be subject to modifying influences, was scarcely considered.

Early experiments on recall did not seriously disrupt this mechanical view. Presupposing its basic validity, workers aimed only at elucidating various aspects of it, and dismissed any incompatible changes in recall as mere distortions of the "normal" memory processes. Such "distortions" were to be avoided at all costs by suitable experimental design—hence the vogue of nonsense-syllable experiments, which left subjects minimum scope for inconvenient responses.

In the reaction against this artificial approach, a prominent part was played by F. C. Bartlett (1), whose experiments emulated as closely as possible the conditions and subject matter of recall in everyday life. He found that expectations from past experience markedly influenced a subject's initial perception of material, and recall was seldom merely reduplicative. He concluded that an adequate theory must accept change in recall as a normal feature of memory and must give the individual an active part to play. To replace fixed traces as the basis of recall, he

¹ Condensed from a portion of the author's doctoral dissertation at the University of Oxford. The author wishes to express his thanks to Professor O. L. Zangwill, who supervised the research and gave invaluable advice and help.

proposed "schemata" or "active organized settings"—broad reaction patterns embodying large sectors of past experience and exerting a mass effect on current perception. New experiences are assimilated into an existing schema and, except for a few outstanding details, lose their specific identity. In recall, an experience is not reproduced, but inferentially reconstructed from the few unassimilated details, in the light of the "schema" as it exists at the time of recall.

Bartlett's work was valuable in drawing attention to the shortcomings of the trace theory in its most mechanical form, but one may suggest that his alternative goes too far in the other direction. The schema theory can explain changes in recall as normal phenomena, but, by eliminating specific traces, it raises afresh the problem of accounting for even moderately accurate recall of particular facts.

The question arises of a possible compromise between facsimile traces which preclude changes in recall and identity-destroying schemata which preclude accurate recall. In postulating traces of some sort to account for specific recall, must one assume that they are in every detail replicas of the original experience? If, as is now generally agreed, perceiving entails interpretation, would not a trace represent an experience as interpreted, and to this extent differ from the ostensible content of the original? Must a trace embody the modality or form of an experience as well as its content or meaning? If not, changes of *form* in recall are compatible with a fixed trace hypothesis. May not traces be so constituted that, without loss of identity, they could also permit certain types of change of *content* in recall? Finally, if traces are accepted as the basis of recall, does it follow that *all* changes of content in recall must reflect changes in the traces? Suitable answers to these questions might lead to a trace theory of memory sufficiently flexible to encompass both the accuracy and the inaccuracy of recall.

Investigators of the recall of meaningful material have used two main experimental approaches, corresponding roughly to the trace and schema approaches on the theoretical side. Workers trained in, or influenced by, nonsense-syllable techniques, which are almost purely quantitative, have tended to apply these techniques when working with meaningful material. Putting connected prose and nonsense syllables on a par—treating both, for analytical purposes, as series of isolable units of equal value—they could easily show that, quantitatively, connected prose is vastly more memorable than nonsense syllables, and infer that the difference is due to the meaningfulness of the former. One cannot go much farther with this approach.

Reacting against the comparative sterility of the gross statistical approach, other investigators justly point out that it ignores the essential nature of meaningful material—the fact that it is *not* a congeries of isolable units but a complex structure, the parts of which are interrelated in a multitude of ways and by no means contribute equally to the whole. This being so, they are tempted to deny that recall of meaningful material is amenable to quantitative analysis in any useful way and to rely too heavily on qualitative analysis.

On the experimental as on the theoretical side a middle course seems desirable to the present writer. The questions to ask are neither purely quantitative nor purely qualitative. In analyzing reproductions of prose material, one should note not only how much of a passage is recalled in relation to original length, but also which parts are retained when length makes omissions inevitable, why these parts are thus singled out, and whether and to what extent selective retention introduces changes in the general meaning of a passage.

With such considerations in mind, the author undertook a series of experiments on the recall of meaningful verbal material. The first, reported in the present paper, is an investigation, on the lines indicated above, of what happens in immediate recall to the content of prose passages of gradually increasing length.

2. METHOD

Material

A series of 37 prose passages, ranging from 13 to 95 words, was used. Content and language were simple in all cases, except that, as the passages were originally assembled for an experiment on rationalizing,² some contained deliberate irregularities of grammar, idiom, or logic. Apart from two descriptive passages, the content was narrative in type. Detailed analysis was made of the results on eight of the narratives and, for comparison, the two descriptions. The former were selected so as to (a) cover the length-range of the series beyond the point where errors in recall became significant, (b) increase in length by a uniform percentage, and (c) include the two narratives closest in length to the two descriptions.

Subjects

A total of 50 subjects participated, half of each sex. Half had only secondary education; half were university students or graduates. Ages

² By T. S. Tufton in 1939, working under the supervision of O. L. Zangwill at the Cambridge Psychological Laboratory.

ranged from 18 to 70, with a median of 30. As sub-groups on the basis of sex, age, and education were too small for the slight intergroup differences to be statistically reliable, the results reported here pertain to the full group of 50.

Procedure

The subjects, interviewed singly, were told that a series of short passages would be read aloud to them and that, upon hearing each passage, they were to repeat orally as much as they could recall, using the original words where possible, but reproducing all retained ideas as best they could. Each passage was then read once, with appropriate expression and at a normal rate, and a running record was made of the subjects' reproductions. The session with each subject took about three-quarters of an hour.

Method of Assessment

In view of the meaningful character of the material, and the fact that a given idea can usually be expressed in several ways, the analysis of the reproductions was concerned primarily with the fate of the various parts of the idea-content of a passage; the specific words used were of secondary importance. The usual type of word-count was made, to show certain quantitative trends in the results and the range of individual differences in performance. For the main analysis, however, which was to emphasize recall of ideas rather than of words, elements in a reproduction were scored as "adequate" or "inadequate" expressions of the ideas in corresponding parts of the original text, not as "correct" or "incorrect" by a verbatim criterion. No problem arose in scoring verbatim elements, full synonyms, complete omissions or obvious non-synonyms. Partial synonyms, displaced elements, and substitute phrases were accepted if the original meaning was still conveyed. The extent to which judgment entered into the scoring may be gauged by the fact that the average set of reproductions gave 55.5 % of the original verbatim, omitted 32.7 %, changed the wording or (far less often) the position of 11.8 %, and added 6.2 %—occasionally extraneous material, but usually just needless repetition or undue verbosity.

The frequency with which a given passage element was "adequately" represented in the reproductions was taken as a measure of its relative mnemonic value in its passage. These frequencies, initially computed as percentages, were converted to ratings on a 5-point scale, to reduce the effects of subjectivity in assessment and to emphasize major differences in mnemonic value by eliminating minor ones. The top rating represented frequencies of 80 % or higher; the second, frequencies between 60 % and 79 %, and so on. Although this method would distinguish unduly between

passage elements lying just on either side of a rating boundary (e.g., with 78 % and 80 % respectively), borderline cases proved to be rare, as were instances where an element's rating could be changed by reversing all doubtful adequacy-judgments. In general, frequency differences between elements in adjoining ratings were great enough to warrant confidence that the ratings represented genuine differences in mnemonic value.

3. RESULTS

Table 1 gives the data on the amount, both absolute and relative, of verbatim reproduction, omissions, changes, and additions in the reproductions of each passage, to show the quantitative trend of each of these recall components with increasing length of passage. Also given are the data on overall performance, separately computed from the individual subjects' scores on the ten passages combined; these data, indicating the relative importance of each recall component in the series as a whole, provide criteria for evaluating the results on any given passage.

The data on "changes" include transpositions as well as substitutions. Transpositions do not bulk large in the reproductions, but account for 35 % of the items listed as "changes," of which 27.3 % are displaced "verbatim" words and 7.7 % displaced substitute words identifiable as such. The "additions" comprise (a) excess words in paraphrases of the original, (b) needless repetitions, (c) connectives inserted to improve logic or style, and—rarely—(d) items not derived, formally or functionally, from any part of the original. Additions form a much smaller group than any other recall component, and the subgroups are far too small for separate statistical treatment.

The columns of mean scores show the general trends that develop with increasing length of passage. In most respects, the first six passages and the last form a regularly progressing series, with passage G and the two descriptions (F_1 and G_1) falling markedly out of line. This appears most clearly in the percentage scores for verbatim reproduction. These, if plotted against the number of words per passage, form a smooth curve descending sharply as far as passage D and thereafter declining at a decreasing rate to E, F, and H. Both F_1 and G_1 fall well below this curve at their respective abscissae, and, indeed, below any point on it, including the point corresponding to the longest of the narrative passages. On the other hand, G shows, for its length, an extremely high percentage of verbatim reproduction, and a higher mean number of words given verbatim than any other passage. Reasons for the aberrance of F_1 , G and G_1 are suggested in the main analysis, where each passage is discussed separately.

The omission scores vary more or less inversely with the verbatim scores but not entirely so, of course, since some of the departures from the original are changes rather than omissions. Changes start at a slightly higher level than omissions, but increase at a much slower rate, and on the longest passage are little more than one-third as numerous as omissions. The change scores are also less consistently related to length of passage;

TABLE 1
*Immediate Reproduction Performance by Fifty Subjects
on Ten Short Prose Passages*

		Recall Component							
		Verbatim Reproduction		Omissions		Changes		Additions	
Passage	Total Words	Mean Score	Stand. Dev.	Mean Score	Stand. Dev.	Mean Score	Stand. Dev.	Mean Score	Stand. Dev.
(In number of words)									
A	18	15.9	2.1	0.9	1.3	1.2	1.7	0.3	0.6
B	23	19.1	2.7	2.8	2.2	1.1	1.8	1.8	2.0
C	31	22.7	4.1	5.3	3.9	3.0	2.4	2.0	2.1
D	37	23.7	6.9	8.9	7.1	4.4	2.3	2.0	2.0
E	45	25.9	6.7	14.0	5.7	5.2	2.4	3.7	3.1
F	58	29.9	8.6	19.6	8.0	8.4	4.2	4.2	3.8
F ₁	52	24.1	7.6	20.7	7.8	7.2	4.1	2.4	2.3
G	71	43.6	9.0	18.5	8.0	8.9	3.7	4.0	3.2
G ₁	75	30.0	12.1	37.9	13.4	7.1	4.0	3.1	3.1
H	86	40.4	10.4	33.5	10.7	12.1	5.8	7.0	4.2
Overall Perform- ance	496	275.4	46.8	162.1	45.6	58.5	18.7	30.5	13.3
(As percentages of total words in passage)									
A	18	88.4	11.8	5.1	7.3	6.4	9.4	1.7	3.6
B	23	83.1	11.7	12.3	9.7	4.6	7.9	7.9	8.6
C	31	73.4	13.3	17.0	12.6	9.6	7.8	6.5	6.9
D	37	64.1	18.7	24.0	19.1	11.9	6.1	5.5	5.5
E	45	57.5	14.9	31.1	12.7	11.5	5.4	8.1	6.9
F	58	51.6	14.8	33.9	13.8	14.5	7.3	7.2	6.5
F ₁	52	46.3	14.7	39.9	15.0	13.8	7.9	4.6	4.4
G	71	61.4	12.7	26.0	11.2	12.6	5.2	5.7	4.4
G ₁	75	40.0	16.2	50.5	17.9	9.5	5.3	4.2	4.1
H	86	47.0	12.0	38.9	12.5	14.1	6.8	8.1	4.9
Overall Perform- ance	496	55.5	9.4	32.7	9.2	11.8	3.8	6.2	2.7

they depend to an important extent on the opportunities a passage offers for synonymic substitution, and these, in turn, depend more on content than on length.

In presenting the results of the main analysis, the original text of each passage is given first, followed by a version in differentiated type, to show the relative mnemonic value of the various parts of the passage as determined by the rating system described above. In this second or "synoptic" version, full-size capitals represent the top rating, small capitals the second, lower case italics the third, and plain lower case the fourth. Fifth rating elements, appearing as asterisks, are included only to show at a glance which parts of a passage have so little mnemonic value that fewer than one subject in five recalls them. The synoptic versions differ textually from the originals in that (a) a particular substitute used by subjects more often than the original word replaces the latter in the synoptic version to indicate the favourite way of expressing an idea, and (b) additions made by 20 % or more of the subjects are included in parentheses, in the style of type appropriate to the frequency of occurrence. Following the synoptic versions are brief comments on the main points of interest as far as concerns abstraction in recall.

Passage A. Wearing his blue jacket and brown rubber shoes, he gave his older cousin a round beating at tennis.

Synoptic version: WEARING HIS BLUE JACKET AND BROWN RUBBER SHOES, HE GAVE HIS OLDER COUSIN A ROUND BEATING AT TENNIS.

This is the shortest passage yielding enough errors in recall to require comment. Only three words, all adjectives, drop below the top rating; the other adjectives are also somewhat weaker than the rest of the passage. In so short a passage, weakness appears more often in the form of a casual treatment of affected elements than in outright omission, and thus foreshadows, without actually producing, abstraction.

Passage B. A surprise awaited us. The children were up, in night-clothes and dressing-gowns. Helped by Cook, they have prepared the breakfast-table.

Synoptic version: A SURPRISE AWAITED US. THE CHILDREN WERE UP, IN NIGHT-CLOTHES *and* DRESSING-GOWNS. HELPED BY (*the*) COOK, THEY *have* PREPARED THE BREAKFAST-TABLE.

More elements drop below the top rating than in passage A and some drop to the third rating, but weak elements are still not numerous. They include the descriptive phrase, frequently telescoped; the relational "by,"

often eliminated by conventionalizing the relation between children and cook; "have," which is in the wrong tense; and "table," which does not usually go with "prepared."

Passage C. The father searched for his son, and not seeing him, went to look in the fish-trap. On pulling it to the river-side, he found a large python in it.

Synoptic version: THE FATHER SEARCHED FOR HIS SON, AND NOT SEEING HIM, WENT TO LOOK IN THE FISH-TRAP. *On PULLING IT to the river-side, HE FOUND A LARGE PYTHON IN IT.*

It is no longer single words that fall below the top two ratings, and there is even one fourth rating element. Weakness is concentrated primarily on the first half of the second sentence, which, being dispensable, is frequently condensed or omitted. The unexpected "fish-trap" and "python" are sometimes rationalized by adding explanatory phrases. Reproductions often employ verbal or syntactic parallelism.

Passage D. I slipped out of my room before the lunch-gong went, to await the carrier. From the shrubbery, I saw Cousin Ambrose enter first my sitting-room and then the bedroom, quite evidently in search of me.

Synoptic version: I SLIPPED OUT OF MY ROOM BEFORE THE LUNCH-GONG WENT, *to AWAIT the carrier. From THE SHRUBBERY, I SAW COUSIN AMBROSE ENTER first my sitting-ROOM and then the BED-ROOM, quite EVIDENTLY IN SEARCH OF ME.*

Passage elements do not tend to fall to lower ratings than were reached in shorter passages, but there is a marked drop in the number of top rating elements, which now include only the key statements. The passage is short enough to permit recall of much of the subsidiary material, but lacks a clear-cut logical basis for selection; with subjects differing in their choice, the result for the group is a mediocre representation for all ancillary items. There is some tendency to make a causal connection between the two main parts that is not present in the original.

Passage E. The postman was on his rounds. As he came up to the door, I ran out crying, "Happy Christmas! Any parcels for me today?" "Two letters, Missy," he said, and seeing my face fall, added slyly, "And a parcel, young lady." I was so excited!

Synoptic version: THE POSTMAN WAS ON HIS ROUNDS. *As he CAME up TO THE DOOR, I RAN OUT CRYING, "HAPPY CHRISTMAS! ANY PARCELS FOR ME today?" "TWO LETTERS, MISSY," HE SAID, AND SEEING MY FACE FALL, added slyly, "AND A PARCEL, young lady," I was so excited!*

Top rating elements convey little beyond the general topic of this passage, but the first and second ratings together include everything of real importance. Several logically important elements with only second rating would probably have got first, but for the fact that many subjects missed the point of the passage (the postman's little joke). A quarter of the text drops to third rating or lower, but items affected to this extent are ones which can be omitted without much loss to the general sense.

Passage F. One of the chicks caught a little frog. He ran round and round holding it by one leg, pursued by all his brothers, cheeping. He dropped the frog. It was picked up by a second chick, who was eventually left alone with his prey unchallenged. But it was the wrong shape to eat, and hopped away quite unharmed.

Synoptic version: ONE OF THE (little) CHICKS CAUGHT A LITTLE FROG. HE RAN ROUND AND ROUND HOLDING IT BY ONE LEG, FOLLOWED BY ALL HIS BROTHERS, cheeping. HE DROPPED the FROG. *It was picked up by a second chick, who * * left * with his prey *. But IT WAS THE WRONG SHAPE TO EAT AND (it) HOPPED AWAY quite unharmed.*

Despite its greater length, this passage has more elements in the top rating, and in the top two combined, than either D or E. This reflects a near unanimity among the subjects in mnemonic selection. The whole third quarter of the passage may be regarded as an intrusive episode, to be dropped at need. It represents about the amount the average subject must omit from this long a passage; if it is dropped, few other omissions are necessary. When retained, this section often appears in a form closely paralleling the opening part of the passage, thus reducing the effort of recall.

Passage F₁. There was a beautiful view. In front of us were great rocks overhanging the precipice. Down below, one saw the river winding like a silver thread across the plain. A little village, like a toy village, lay directly underneath us, and we could see thin coils of smoke rising from the chimneys.

Synoptic version: THERE WAS A BEAUTIFUL VIEW. *In front of us were great ROCKS overhanging the precipice. DOWN BELOW we could see THE RIVER WINDING LIKE A SILVER THREAD across the valley. There was A LITTLE VILLAGE, like a toy village, * underneath us, WITH * coils of SMOKE RISING FROM THE CHIMNEYS.*

Subjects omit substantially more of this descriptive passage than of passage F, the narrative of comparable length; the general level of

representation is correspondingly lowered. There is also a more even distribution of ratings than in F, because, with no real chain of events to serve as a guide, the retention of passage-elements apart from the mere names of the objects described is largely optional, and subjects differed in their selection. An interesting feature of many reproductions is a tendency to reword in such a way as to create a quasi-narrative effect without actually changing the meaning.

Passage G. He was woken at six by a bell, to find a big mottled spider on his forehead, and two green beetles in his bedding, besides some earwigs, which scuttled away when he moved. He shuddered. Nor did he enjoy walking barefoot over the chilly grass, to wash in ice-cold water in a tin basin. The other campers were very jovial, and said that he would soon get used to things.

Synoptic version: HE WAS WOKEN AT SIX BY A BELL, TO FIND A BIG MOTTLED SPIDER ON HIS FOREHEAD, AND *two* GREEN BEETLES IN HIS BEDDING, *besides* some *earwigs*, which scuttled * when he moved. He shuddered. HE DID NOT ENJOY WALKING *bare-foot* OVER THE *chilly* GRASS, TO WASH IN ICE-COLD WATER *in a tin basin*. THE OTHER CAMPERS *were very jovial*, and SAID THAT HE WOULD SOON GET USED TO IT.

In view of the length of passage G, the preservation of content in the reproductions is remarkable; over half of all elements get top rating, and the top two ratings cover two-thirds of the text. Similar personal experiences, allowing subjects to identify with the central character of the piece, seem to be an important factor in the superior retention. The large proportion of high rating elements is also partly the result of concentrating omissions on the short second sentence and the end of the first, the latter being superfluous detail and the former readily inferred. Throughout, descriptive elements are less well represented than the rest of their immediate context.

Passage G₁. She rode on through the little old town with its quaint shops and signs, and down the hill into the rolling country-side, with miles of pasture-land visible on either side, with the little low hedges and quiet grazing cattle. The hills on the left were just showing through the mist. She cycled on through twelve miles of this, and then arrived at the hostel just by the crooked stone bridge over the stream.

Synoptic version: SHE RODE ON THROUGH THE LITTLE *old* TOWN, *with its* quaint *shops* and signs, AND *down the hill* into THE rolling COUNTRY-side, *with* miles of *pasture-land* * on either side, *with*

the little *low hedges and quiet grazing cattle*. The hills * * * * * showing through the mist. SHE CYCLED ON *through twelve miles* of this, and then (she) ARRIVED AT THE HOSTEL just by the crooked stone bridge over the stream.

This is the second descriptive passage, comparable in length with G; comparison of the results shows how markedly factors other than mere length may affect recall. Only half of the original text is represented in the average reproduction of G_1 , against three-quarters in the case of G. There is even greater disparity in the distribution of mnemonic value ratings in the two passages: where the top two ratings covered two-thirds of G, they cover less than a quarter of G_1 , and—significantly—include only its meagre narrative content. As in F_1 , individual differences in selecting among the descriptive items result in moderate representation for each of the major ones. Adding these third-rating elements to those in the top two ratings gives a fair outline of the content of the original, but over two-fifths of the passage drops to fourth rating or lower. The tendency noted in passage F_1 to turn description into quasi-narrative where possible appears in G_1 too, especially in the more successful reproductions.

Passage H. Every single one of the party was drunk. As they left the building, Fred called Neddy a liar, and received a similar compliment in return. Ned landed Freddy one. He collapses and lays quite still on the curb. A policeman came up, seized them by their collars, shook them, and took them off to cells for the night. Two mates of his dealt likewise with the rest, who had stayed behind to watch the fray, and had been waking several neighbours up with their loud guffaws.

Synoptic version: EVERY *single* ONE OF THE PARTY WAS DRUNK. AS THEY LEFT THE BUILDING, FRED CALLED NEDDY A LIAR, AND (*Neddy*) RETURNED THE COMPLIMENT. *Ned* LANDED *Freddy one*. He collapsed * * * * ON THE PAVEMENT. A POLICEMAN CAME UP, SEIZED THEM by their collars, * *, AND TOOK THEM OFF TO CELLS FOR THE NIGHT. *Two of his mates dealt likewise with the rest* (of the party), who had stayed behind to watch the fray, and * * waking the neighbours * with their loud guffaws.

The content is well retained for so long a passage, though not to the extent found in G; fewer subjects could make a personal identification, but the action is exciting enough to hold attention and so aid recall. As regards idea representation, the fact that the last third of the passage is not immediately relevant to the main story and can be omitted permits most of the rest, apart from minor details of the fight and arrest, to get

first or second rating. The third sentence is rather weakened by the confusing treatment of names. In the last part, portions analogous with, or directly related to, the main story get third rating; the rest drops to the lowest ratings.

4. DISCUSSION

The most striking feature of the synoptic versions is the highly *selective incidence* of omissions, as indicated by relatively low representational ratings, and of passage elements represented in all or nearly all reproductions. In short passages, of which most subjects can reproduce nearly all, the few words omitted are those which, if retained, contribute least to the general meaning of the passage and whose loss therefore does least damage. As the passages lengthen, omissions progress from single adjectives, through short descriptive phrases, to longer phrases which are only incidental to the main theme. In the longest passages, omissions are no longer confined to modifiers of single words, or to phrases of little importance beyond their immediate context, but eliminate whole sentences or even whole sections introducing secondary themes.

In general, as more and more must be dropped, the criteria of importance in the passage become increasingly stringent. A contribution to the general meaning that would ensure retention in reproductions of a short passage may well not suffice in a long passage from which the average subject must drop a third or half. At whatever cost to the rest, however, the key parts of a passage, giving its general theme, are preserved in some form in the reproductions; the only exceptions in the entire series are a very few cases where, for some reason, a subject clearly failed to understand the passage in the first place.

The progression of omissions as passages lengthen is seen also in going from the fragmentary to the extensive reproductions of any given passage. The latter omit, if anything, only insignificant details; the former may retain only one or two key phrases. But, whereas the synoptic versions summarize the responses of the whole group and so reflect quite accurately the relative contributions of passage-elements of intermediate importance as well as those of very great or very little importance, the individual reproductions of given passages show considerable variation among the subjects in evaluating elements of intermediate importance, as judged by their omission or retention. This is particularly noticeable in those passages where, apart from a few quite skeletal key phrases, distinctions of importance are slight; here almost any combination of passage-elements making

sense in itself and compatible with the general theme of the passage is likely to be used by one subject or another. The retention or omission of such intermediate items is, almost by definition, largely optional; it is the fact that individuals differ in their unwitting exercise of this option that enables the synoptic versions to distinguish intermediate levels of importance.

If omissions merely increased in number as passages lengthened, affecting parts more or less at random, they would reveal little about the processes involved in the recall of meaningful material. But the close correspondence between an element's contribution to the total meaning of a passage and the frequency with which it is represented in reproductions shows clearly that the omissions are not fortuitous but reflect an active process. The selection and organization of stimulus material by this process is essentially abstractive in effect; since its results can be noted only in recall, we may term it the *abstractive process in recall* or, more briefly, mnemonic abstraction.

The selectivity of omission and retention is not in itself a new finding, though the "synoptic versions" bring out the phenomenon more clearly than other means that have been tried. It can hardly escape notice when any attention at all is paid to the qualitative aspects of meaningful recall. Binet and Henri (2) seem to have been the first to comment on the phenomenon. It was later encountered by Henderson (5) in an experiment on repeated reproduction, and by Hartgenbusch (4) in one on serial reproduction. Lewis (6), using stories containing 37.5 % of deliberately irrelevant details, reported that these irrelevancies accounted for 63 % of all omissions in reproductions.

None of these writers, however, satisfactorily explained this selectivity. Binet and Henri suggested that the pattern of selection reflected fluctuations coinciding with differences in relative importance of the parts of a passage, which they apparently regarded as too self-evident to require analysis. Henderson held that details are recalled with the aid of a "sense of the general meaning" which is itself "immanent" in the details, while Lewis invoked the Gestalt concept of "persistence of the organized whole"—both views tantamount to restating the phenomenon rather than explaining it. Hartgenbusch offered no explanation at all.

The present experiment provides a basis for advancing at least a step farther in explaining selective recall. First, it demonstrates the *immediacy* of mnemonic abstraction. Unlike some experiments, in which "immediate" reproductions were delayed up to half an hour, in this one no time elapsed between readings and reproductions. Yet the selection shown in

the reproductions plainly implied that the parts of a passage had *already* been seriated in order of importance. The abstractive process apparently did most of its work before the reproductions were begun. Hesitation and self-correction usually meant an attempt to follow instructions and give a *verbatim* rendering of ideas that were recalled and would be reproduced in some form in any case; the infrequent pauses in an effort to recall more of the original *content* concerned marginal ideas only slightly more important than the ones unwittingly omitted outright. In general, the central train of thought of a passage was reproduced rapidly, confidently, and accurately enough as to sense, though by no means always verbatim.

Insofar as it can be seen in immediate recall, the abstractive process seems to develop concurrently with the process of *understanding* a passage while hearing it. These two processes share at least one important feature: the selectivity of mnemonic abstraction implies an unwitting ranking of parts according to importance; full understanding of a passage also demands an appreciation of the relative importance of its parts. However, mnemonic abstraction, even in its immediate operation, goes beyond mere understanding, for, after the ranking of parts, it proceeds to eliminate enough of the least important ones to bring the content within the scope that can be encompassed in recall in the prevailing circumstances.

This eliminating function of mnemonic abstraction is not always apparent in immediate recall—there is no outward sign of it in full verbatim recall of short passages; and when passages of any length are especially interesting, it operates less ruthlessly than one might anticipate from the length alone. Whether, and to what extent, mnemonic abstraction will appear in recall seems to depend largely on the *distribution of attention*. External dispersal of attention, due to irrelevant preoccupations and distractions, is, of course, minimized in an experimental setting, and is further reduced in proportion to the subject's interest in the task. But, however successfully attention is concentrated on the task, it is inevitably dispersed over an ever wider range of content as the passages lengthen. If attention were evenly attenuated over all parts of a passage, we should find, at some point in the length-series, a sudden shift from full recall to no recall at all: this does not occur. The alternative was suggested by Binet and Henri: attention fluctuates during the hearing of a passage, certain parts drawing "maximum attention" at the expense of others, which thereby become inaccessible to recall.

If this is so, whatever causes the fluctuations in attention also determines the idea selection in mnemonic abstraction. (There is no reason to suppose,

be it noted, that subjects consciously direct these fluctuations, or are even aware of them.) This raises again our earlier question: on what basis does mnemonic abstraction select? In a sense it is a circular argument to say merely that the "more important items" are recalled, the "less important" omitted. An item is "important" precisely *because* subjects react strongly to it and are thus able to recall it—not the other way about. Recognition of this fact calls for a reformulation of the question, which becomes: what *are* the "important" parts of a passage?, with "importance" defined in terms of relative strength of reactions to the parts of a passage as gauged by relative frequency of use in reproductions.

Several features of the experimental results are suggestive in this connection: (a) In the narrative passages, purely descriptive elements tend to be weaker than the rest of their context. (b) Reproductions of the descriptive passages were, with few exceptions, much poorer in content than those of narratives of comparable length. (c) In general, verbs were the best represented type of element, though less likely to be rendered verbatim than, say, nouns, because synonymous expression was more often possible. (d) Subjects took advantage of real or potential narrative elements in descriptive passages, so that in G_1 only the meagre narrative content got top rating representation, and, wherever possible in both F_1 and G_1 , many subjects achieved a narrative effect by expressing the content in terms of "doing" rather than "being" something. In short, the strongest reaction was to elements bearing the action content of a passage. Agents had the next best representation, followed by the effects or recipients of the action. The weakest reaction was to items which, while serving to fill in the general picture, only retarded the action. The agent-action-effect unit, or a related chain of such units, may be termed the "figure" of a passage, the rest comprising the "ground."

Such a pattern suggests how mnemonic abstraction may proceed. At the outset, until the tenor of a passage becomes apparent, attention is paid to everything. The opening words are thus likely to be well recalled, whatever their nature. The first action-word gives a clue to what is to happen, and is also the nucleus for the figure of the passage, or at least its first part. The rest of the figure is quickly picked out, ground elements remaining in abeyance on the edge of focal attention. In a short passage, not only the figure but also much of the ground can be recalled. In longer passages, additional figures, related to the first, push ground elements out of focal attention and render them unavailable to recall, leaving, in the most skeletal reproductions, only a series of agent-action-effect units with no modifying elements. But all elements must get some passing attention,

if only to be recognized as "figure" or "ground." The ground elements may, before being discarded, receive enough attention to enable them to colour the interpretation placed on the figure, so that, in recall, the latter is rendered by partial synonyms incorporating part of the idea-content of the vanished ground elements.

This picture of the organization of attention around the action content of a passage can, obviously, apply in full-fledged form only when the passage has a substantial narrative content. However, attempts to apply this method where it is not strictly applicable, and the inferior memorability of material to which it cannot be applied, suggest that it is the naturally preferred and most effective method of organizing attention and, thereby, the abstractive process seen in recall. The interpretation offered here may seem at first glance to leave little room for the important rôle which affective factors are now generally believed to play in memory (8). However, at least one writer—Müller-Freienfels (7)—maintains that the affective factors operative in memory are in large part kinaesthetic or "motoric" ones. The present experiment supports this view, the results suggesting that we react to, and recall, vicarious experiences in the form of verbal symbols much as we react to "real" experiences, in which actions are normally more effective stimuli than static conditions.

SUMMARY

The paper reports the results of an experiment on the treatment, in immediate recall, of various parts of the content of prose texts exceeding the memory span and the way this treatment varies with increasing amount of material to be recalled.

The experimental material comprised 37 prose passages, 13 to 95 words in length, of which immediate oral reproductions were obtained from 50 subjects. A detailed analysis is given of the results on ten passages—two descriptions and eight narratives.

In view of the meaningful character of the experimental material, the main analysis is based on an assessment of the reproductions in terms of adequacy of representation of the ideas in the original texts, regardless of the exact wording used. The several components of a passage were given "mnemonic value" ratings according to the frequency of adequate representation in the reproductions.

Analysis of the representation of passage content showed a high selectivity in the incidence of omissions. Items omitted were regularly those which contributed least to the general meaning of the passage; whatever proportion of a passage could be reproduced, it included the parts which conveyed to the maximum extent the general meaning of the passage as understood by the subject, though not necessarily in the original words.

The selectivity of omissions was taken to reflect the operation of an unwitting abstractive process whereby the parts of a passage are seriated in order of relative

importance and enough of the least important ones eliminated to bring the content within the scope that can be encompassed in recall.

It is suggested that the abstraction is effected by means of fluctuations in attention during the hearing of a passage, attention being drawn most strongly to parts embodying the action content, and to a lesser extent to other parts, roughly in proportion to the closeness of their relationship to the chain of action. The importance of action content in mnemonic abstraction is suggested not only by its prominence in reproductions, but also by the relatively poor retention of passages which lack a clear chain of action and the tendency of many subjects to attempt to turn descriptive passages into quasi-narratives.

ZUSAMMENFASSUNG

Diese Abhandlung berichtet die Resultate eines Experiments über die Behandlung in unmittelbarem Gedächtnis der verschiedenen Teile des Inhalts einiger die Gedächtnisspanne überschreitenden Prosatexte und die Veränderung dieser Behandlung bei steigendem Ausmasse von Material zur Erinnerung.

Das Versuchsmaterial besteht aus 37 Prosatexten in der Länge von 13 bis 95 Worten, von welchen sofortige mündliche Reproduktionen von 50 Versuchspersonen erhalten wurden. Die Ergebnisse von 10 dieser Prosastücke, d.h. von 2 Beschreibungen und 8 Erzählungen, sind aufgenommen worden.

Im Hinblick auf den sinnvollen Charakter des Versuchsmaterial, gründet sich die Hauptanalyse auf die Auswertung der Wiedergaben, soweit sie die Zulänglichkeit der Darstellung der Ideen in den Originaltexten betrifft, unabhängig von der dort angewendeten Ausdrucksweise. Für jede Komponente eines Stücks wurde der „mnemische Wert“ bestimmt auf Grund der verhältnismässigen Häufigkeit von genügenden Darstellungen in der Wiedergabe.

Die Analyse der Wiedergabe des Inhalts offenbarte eine hohe Selektivität bei Auslassungen. Die ausgelassenen Einzelheiten waren regelmässig jene, welche am wenigsten zu der allgemeinen Bedeutung des Stücks beitrugen; was alles in einer Reproduktion wiedergegeben wurde, enthielt die Teile, welche im Höchstaussasse die allgemeine Bedeutung des Stücks in der Auffassung der betreffenden Versuchsperson wiedergaben, wenn auch nicht durchaus mit dem Originalwortlaut.

Die Selektivität der Auslassungen führte zur Annahme der Wirkung eines unbeabsichtigen abstrahierenden Prozesses, wodurch die Teile eines Prosastücks ihrer relativen Bedeutsamkeit nach geordnet sind, und eine genügende Anzahl von den am wenigsten bedeutsamen ausgelassen wurden, um den Inhalt innerhalb des Bereichs des Erinnerungsvermögens einzuschränken.

Es wird vermutet, dass diese Abstraktion durch Aufmerksamkeitsschwankung während des Anhörens eines Stücks bewirkt werde: die Aufmerksamkeit wird am stärksten auf die Teile, die die Handlung beinhalten, gelenkt, und in geringerem Ausmasse auf die anderen Teile, ungefähr im Verhältnis zu der Nähe ihrer Beziehung zum Leitmotiv der Handlung. Die Annahme der Bedeutsamkeit des Handlungsinhalts für die mnemische Abstraktion ist nahegelegt nicht nur durch sein Hervortreten in der Reproduktion, sondern auch durch die verhältnismässig schlechte Beibehaltung jener Stücke, welchen es an einer klaren Handlungskette fehlt, und durch die Neigung vieler Versuchspersonen, zu versuchen, beschreibende Stücke in eine Art von Erzählungen zu verändern.

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THE INHERITANCE OF EXTRAVERSION-INTROVERSION

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1. INTRODUCTION

The data reported in this paper formed part of an investigation conducted under the writer's direction by Dr. H. McLeod and Dr. D. Blewett from 1951—1953. This investigation was in part made possible by a grant from the Eugenics Society. Some of the results have been reported in Ph. D. theses (2, 16) and in article form (3).

The investigation as a whole was designed to answer a number of different questions, some of which only will be discussed in this paper. In essence we shall be concerned with two closely related problems. The first of these is the factorial definition and measurement of the personality dimension or continuum known as extraversion-introversion; the other is the discovery of the degree to which heredity plays a part in determining a person's position on this continuum. Most of the work on extraversion-introversion has been done with adult subjects; in this study we shall be concerned with school children, mostly of an age between 145 and 185 months.

A number of questions arose in the course of the investigation, or were from the outset considered to determine the design of the experiment. These additional questions, such as, for instance, the relationship between extraversion-introversion and Rorschach's concept of the extratensive/introvertive type of personality, will be discussed as they arise in the course of this paper.

2. THE PROBLEM OF MEASUREMENT

A considerable amount of experimental material relevant to the measurement of extraversion-introversion has been discussed in previous publications by the present writer (5, 6, 10). By and large the results reported there have shown that there is experimental evidence in favour of the existence of some such personality continuum as Jung postulated,

at least among adults; that this dimension can be found, both among normal and among neurotic subjects; and that a variety of different tests could be constructed to measure this dimension with different degrees of reliability and validity. It was further found that, as Jung had postulated, extraverted neurotics tended to develop hysterical or psychopathic symptoms, whereas introverted neurotics tended to develop dysthymic symptoms, such as anxiety, reactive depression, or obsessional features. None of the studies carried out in this laboratory, or available in the literature, had concerned themselves with measurement of extraversion-introversion in children. Consequently it appeared worth-while to test the hypothesis that behavioural relationships similar to those found among adults could also be found among children to define an extravert-introvert continuum.

Among the types of measures used with adults had been objective behaviour tests, ratings, and self-ratings, and it seemed desirable to include these divergent types of measures in the children's study also. In addition, however, it was decided to include a rather different type of test, namely, the Rorschach. Although the writer has been somewhat critical of its use as a "global" measure of personality, some attempts made by members of the department had indicated that when scores on this test are used in the usual psychometric manner, meaningful relations can be established, although (or possibly because) the test thus loses its subjective and interpretive character (4). The main reason for introducing the Rorschach into the experiment was, of course, the fact that Rorschach's theory contains the concept of the opposed types of the "extratensive" and the "introvertive" person. Although Rorschach workers often deny that these terms are co-extensive with Jung's typology, nevertheless it seemed a reasonable hypothesis to expect a considerable degree of similarity. Curiously enough no test of this hypothesis had ever been carried out previously to our knowledge, and consequently a number of Rorschach scores were included in our battery.

In addition to the variables discussed so far, we also included a battery of intelligence tests and a battery of autonomic measures. There are two main reasons for the inclusion of the battery of intelligence tests. In the first place, some at least of the tests used for the measurement of extraversion were known to be also measures of intelligence. Without the inclusion of reliable and valid measures of intelligence, therefore, contamination between the effects of extraversion and those of intelligence might easily have taken place. This is particularly obvious in the case of some of the Rorschach variables. Thus, for instance, a high movement score on the

Rorschach, according to Klopfer, indicates high intelligence. It also, however, indicates introversion. Assuming, for the moment, both these hypotheses to be true, before using the $M\%$ score as a measure of extraversion, we would have to partial out that part of the variance assignable to intelligence.

The second reason for including tests of intelligence in our battery was as follows. Most of the work on the inheritance of intelligence has made use of a single test. This does not seem permissible as Eysenck and Prell (11) have argued in a recent paper, because the fact that the score on a given test has a high h^2 when a comparison is made between the scores of identical and fraternal twins, is indeterminate as long as we have no way of assigning the hereditary component indicated in this way to a specific part of the factor variance.¹ Thus, for example, if the Binet test were found to give much higher intra-class correlations for identical than for fraternal twins, we would still not know whether the hereditary influence thus indicated affected the general intellectual ability measured by the test, or the verbal ability also measured, or the numerical ability, or any of the other factors contributing to the total variance. The conclusion reached by Eysenck and Prell was that it is not test scores which should be submitted to such analysis but factor scores, and accordingly a number of intelligence tests were included here to make possible such an analysis of factor scores.

Also included were a number of autonomic measures, such as systolic and diastolic blood pressure, pulse rate in the resting state and under stress, sub-lingual and finger temperature, and dermatographic latency. The main reason for the inclusion of these measures was as follows. In "The Structure of Human Personality" (8) a number of studies have been summarized suggesting that autonomic lability may be related to neuroticism. If this were true, then it should follow that autonomic measures of this type should correlate with measures known to be good indicators of neuroticism, such as, for instance, body sway suggestibility. Thus, if autonomic measures and a few known tests of neuroticism were included, and if the theory were to be substantiated by our research, then we would expect, in addition to a factor of extraversion-introversion and a factor of intelligence, also to find a factor of neuroticism containing some, if not all, of these autonomic tests. In this way it was hoped to extend the work begun by Eysenck and Prell in 1951 (11).

¹ h^2 is the symbol used by Holzinger to denote a statistic proposed by him as a measure of the degree of hereditary determination of a given trait or ability.

For a critical discussion of it, cf. May (17).

The actual tests and measures included in this study will be described briefly in the third section; a much longer description will be found in the theses by McLeod and Blewett (16, 2). In most cases the rationale for including a test has not been given here because considerations of space make this impracticable. A thorough documentation can be found in the writer's previous summaries of work done on these problems. Quite generally it may be said that a test was included as a possible measure of introversion-extraversion when it either had in the past been found in factorial analyses to have significant projections on this factor among adults, or when it had in the past been found to differentiate significantly between hysterics, the neurotic prototype of the extravert, and dysthymics, the neurotic prototype of the introvert. This would, of course, be reasonable only on the assumption that the behaviour of children and their responses to the test situation are similar to those of adults. This assumption appears to be reasonable and, as will be seen in the section on Results, is, in fact, borne out.

3. THE SAMPLE STUDIED

Little need be said here as in all essentials this study is a duplication of the Eysenck-Prell study. We have relied again on the differences found between identical and fraternal twins to give us evidence regarding the hereditary determination of any particular test score or factor score used in the investigation. [The general theory is too well-known to be discussed in any detail: it depends on the fact that differences between identical twins must be due to environment; differences between fraternal twins may be due to either environment or heredity. If, therefore, differences between identical twins and differences between fraternal twins are equal in size, the total variance of the particular test under investigation can be ascribed to environmental influences. The greater the similarity of identical twins as compared with fraternal twins, the greater will be the amount of hereditary influence it is necessary to postulate.] A convenient formula to assess the amount of hereditary influence has been given by Holzinger. His statistic, which he calls h^2 , has frequently been criticized. A general discussion of the twin method, the difficulties which it gives rise to, and possible criticisms of it is given elsewhere (11), and a discussion of Holsinger's h^2 statistic will be found in another paper from this department (17).

The exact details of the population of children used in the present study have been published by Blewett (3). Here it is merely necessary to summarize the main points. Our sample was drawn from four metro-

politan boroughs in South London. Our thanks here are due to the co-operation of the London County Council who wrote to headmasters of all the L.C.C. secondary schools in the boroughs of Camberwell, Southwark, Lambeth and Lewisham, requesting a report on any twins on their registers. 102 pairs of twins were located, of whom 56 pairs were subsequently tested. Four of these were later dropped on a random basis to equate numbers of pairs in the four groups: male identical, female identical, male fraternal, and female fraternal, retaining 13 pairs in each group. A thorough check was carried out to avoid various well-known sources of error in the selection of the sample; these are discussed in detail by Blewett.

The criteria used in this study were practically identical with those used by Eysenck and Prell, including rating scales for closeness of similarity of facial features, general habitus, hair colour and distribution, iris pigmentation, shape of ears, and teeth. Height and weight were measured and the ability of the subjects to taste phenyl-thio-carbamide was established. In addition, blood groupings and finger-prints were taken into account. Again, details are given by Blewett (3) and there is little doubt that the final decision regarding the zygoticity of the twin pairs arrived at on the basis of all these criteria is essentially correct. The mean age of the children tested was 166 months, with a standard deviation of 11 months. Age was partialled out from the intercorrelations in the factor analysis as it seemed essential to have data not contaminated by this variable.

4. TESTS USED

The tests used in this investigation will now be briefly described. In connection with each will be given an index which will enable the reader to identify it in the factor analysis. The first two variables included in the factor analysis are zygoticity (index number 1), and sex (index number 2); these are not exactly tests in any sense of the word, but are referred to here, nevertheless, in order to keep all the index numbers together. The scoring in these cases was as follows: zygoticity — $M = 1$, $D = 2$; sex — $M = 1$, $F = 0$.

Next we have the set of intelligence tests included in this investigation. Most of these were taken from Thurstone's tests of primary mental abilities for ages 11—17. These are so widely used that it would serve no useful purpose to describe them in detail. The directions given in the Revised Manual (1949) were followed in the administration, and Thurstone's scoring methods were used throughout. The particular tests used

were the verbal scale (index number 8), the numbers scale (index number 9), the space scale (index number 16), the reasoning scale (index number 17), the fluency scale (index number 18) and the total score (index number 19), calculated according to Thurstone's formula:

$$V + S + 2N + 2R + W.$$

In addition, we used the Furneaux level and speed tests. These are described in some detail by Eysenck (7) and by Blewett (3).

Our next set of scores is derived from the Rorschach test. Standard methods of administration, enquiry, and testing the limits were employed. We followed the method outlined by Klopfer and Kelly (15). The following scores were used: Popular responses (index number 28), average response time (index number 29), D (index number 31), $To \div de$ ($H + A \div Hd + Ad$) (index number 32), $FM \div M$ (index number 33), $F \%$ (index number 34), $M \%$ (index number 36), $FM + m - Fc + c + C'$ (index number 37), range of response times (index number 13), and lastly a composite score of pathological indicators devised by Blewett and given in detail in his thesis (index number 30). Most of these variables had odd and abnormal distributions and had to be transformed in various ways, usually by a logarithmic transformation.

Also included with the Rorschach group might be another test, the Rosenzweig Picture Frustration test, as this too is often considered as a projective technique. The only score used here was the extrapunitive one (index number 35).

The autonomic tests employed were as follows: Systolic blood pressure (index number 39) and diastolic blood pressure (index number 40). (Room temperature and humidity were measured at the time this and the other autonomic tests were administered, and wherever a significant relationship was found, temperature and humidity were partialled out.) The other measures used were pulse rate after stress (the stress consisted of pulling a hand dynamometer ten times as hard as possible) and pulse rate after resting (index numbers 41 and 42). Sub-lingual temperature (index number 43) and finger temperature (index number 44) were also taken. Lastly, dermatographic latency (index number 35) was determined using Wenger's method (23).

The next set of variables consisted of ratings and sociometric measures. Questionnaire scales were used, both in the form of self-assessments and teachers' assessments. The scales used were adaptations of Guilford's C and R scales, which have been shown to be good measures of neuroticism and extraversion respectively (8). The detailed scales employed are given in the theses by Blewett and McLeod respectively (2, 16). Based

on these scales, then, we have a teacher's rating of extraversion (index number 4), a teacher's rating of neuroticism (index number 15), self-ratings of extraversion (index number 5), and self-ratings of neuroticism (index number 7). A lie scale based on the well-known M.M.P.I. — but adapted for use with children — was also employed (index number 6).

Two sociability scores were obtained, both derived from a sociometric examination. The subjects were asked simply to write down names of their choice to a series of questions. These questions were of the following kind: "Whom would you like to sit by during class?" "Who do you think would choose you to sit beside them in class?" "Whom would you like to be with after school?" and so forth. The two scores were the total number of names given (index number 53) and the total number of *different* names given (index number 54). The hypothesis underlying this test was, of course, that extraverts, being more sociable, would give a larger number of names in both categories.

The last set of tests to be considered consists of objective behaviour tests. The first of these is the body sway test of suggestibility (index number 11); the second, the finger dexterity test (index number 14). Both these tests are described fully in "The Scientific Study of Personality" (6). Next, we have three tests of rigidity taken from the work of Ferguson and his colleagues (20). These are the opposites test (index number 22), the alphabet test (index number 23), and the arithmetic test (index number 24). These tests are based on the interfering effects of highly habituated culturally induced behaviour patterns in tasks involving largely cognitive processes. Another index of rigidity, called the index of flexibility, is a measure of the amount of change in level of aspiration by actual performance (index number 25). It is taken from a test using the so-called triple tester described in "The Scientific Study of Personality" (6), as is the affective discrepancy score (index number 50) which is the sum of the goal discrepancy and the judgment discrepancy scores. The rationale and meaning of these scores are discussed in "Dimensions of Personality" (5).

Two tests of persistence were included, namely, the leg persistence test (index number 26) and the dynamometer persistence test (index number 27). Both tests have been described in previous publications. As a test of expressive movement two of Mira's (19) tasks were used, namely, the drawing of sagittal lines and the drawing of vertical lines. The score on this test was the total area covered by the lines (index number 38). Two tests of humour were included, one of orectic (index number 46) and one of cognitive (index number 47) humour. The test consisted of 30 cartoons which had to be rated with respect to the amusement derived from them;

the rationale for this test is given in "Dimensions of Personality" (5).

The Porteus Maze test was also given to the children. As Hildebrand (13) and Foulds (12) have shown, certain qualitative performances differentiate hysterics from dysthymics. Included in our study, therefore, were scores "wrong directions" (index number 48) and "lifted pencils" (index number 49). Two scores were also taken from the track tracer described in "Dimensions of Personality" (5). One of these is an accuracy score, the other one a speed score (index numbers 51 and 52).

Last of all, a score was included consisting of the level-speed discrepancy on the Furneaux test (index number 21). Here a high score indicates a lack of such discrepancy; in view of results reported by Eysenck (7), this may be regarded as evidence of normality.

5. RESULTS

Variables indexed in the section above were intercorrelated, the effect of age was partialled out from the intercorrelations, and a factorial analysis undertaken of the resulting matrix. In order to avoid subjective determination of axis rotations by the writer, the rotations were carried out in the statistical section of the writer's department under the direction of Mr. A. E. Maxwell. The results are therefore not influenced by the writer's own conceptions, although this may, of course, intrude in the interpretation of the results given later on. However, the reader will be able to check these interpretations against the figures. Table 1 gives the factor saturations for the 52 variables on the 6 factors extracted, as well as the communalities. The peculiar constitution of the sample, i.e. the fact that it is composed of closely related subjects, makes it impossible to apply any known tests of significance to the residuals, and we have probably erred in taking out more factors than is warranted. However, no interpretation is here attempted of the last three factors, and those with which we shall be concerned are indubitably both significant and meaningful.

The main loadings on factors 1 and 2 have been plotted in Figure 1 and it will be seen that we are dealing essentially with the factors of intelligence and extraversion-introversion. The identification of the intelligence factor leaves very little room for doubt. The Thurstone total score has a loading of .947. All the other Thurstone scores have appropriately high loadings (verbal = .695; number = .569; space = .635; reasoning = .821; frequency = .629). The two Furneaux scores had loadings of .529 and .677. Finger dexterity, as is reasonable with children, has a loading of .389. Two of the rigidity tests have high loadings; the opposites

TABLE I

Variable:	I	II	III	IV	V	VI	h^2
22	.579	-.103	-.055	.091	.019	-.034	.359
14	.389	.012	-.215	.037	.099	-.016	.209
8	.695	-.016	.014	-.181	-.050	.062	.523
11	.258	.090	.061	-.162	-.062	-.025	.109
20	.529	-.006	-.041	-.293	-.092	-.042	.378
16	.635	.105	-.066	.106	.037	-.033	.432
19	.947	.075	.124	.036	-.015	.075	.925
10	.677	-.019	.291	.285	.101	-.086	.642
18	.629	-.071	.225	-.057	-.048	.092	.465
9	.569	.096	.200	.047	.025	.162	.402
40	.232	-.230	.452	.015	-.014	-.046	.314
17	.821	.095	.123	.198	.040	-.021	.739
23	.656	.151	.264	.197	-.011	.038	.563
6	.374	.200	.109	-.301	-.046	-.015	.285
34	-.296	.286	.095	.089	-.124	-.067	.206
24	-.161	-.106	.219	-.140	-.010	-.099	.115
48	-.448	.013	.110	-.231	-.021	.071	.272
51	-.389	.162	.041	-.016	.006	-.098	.189
15	-.159	.165	-.177	.084	.021	.005	.091
44	.031	.300	-.181	-.066	.056	-.023	.132
33	-.090	.501	-.292	.137	-.013	-.046	.365
1	.167	.217	-.166	-.226	.057	-.010	.157
46	-.149	.162	-.026	-.358	-.077	-.002	.183
28	.095	.242	.107	-.227	.023	.106	.142
31	.164	.510	.106	-.192	.047	.077	.343
26	-.004	.229	.129	-.087	.050	.167	.107
53	.073	.632	-.017	.061	.011	-.067	.413
54	.121	.574	-.094	.094	.029	-.083	.370
43	-.121	.200	.620	.034	.011	.096	.450
36	.191	-.626	.175	-.084	-.030	.014	.467
52	-.098	-.378	-.047	-.171	.050	.115	.200
30	.013	-.396	-.022	-.272	.096	-.078	.247
32	.112	-.189	-.191	-.264	-.013	.050	.157
38	-.045	.027	.184	-.215	.051	-.087	.088
49	-.148	-.111	.177	-.250	-.092	-.049	.137
42	.076	-.066	.913	.148	.006	-.026	.894
2	.065	-.057	.855	.122	-.054	-.025	.781
29	-.079	.032	.162	.594	-.126	-.016	.402
5	-.104	-.091	-.282	-.030	-.135	.011	.118
27	-.020	.013	-.197	.141	-.037	.127	.077
50	.124	.007	-.447	.123	-.164	.009	.257
21	-.136	-.015	-.240	-.466	-.172	.035	.324
45	.193	-.002	-.216	-.233	.023	-.049	.141
47	-.096	-.056	-.192	-.406	.001	-.096	.223
13	-.006	-.100	.109	.530	-.118	-.024	.317
7	.076	.095	.140	-.350	-.073	-.023	.163
4	.042	.176	-.042	-.326	.073	-.045	.148
35	.096	.119	.103	-.228	.021	.024	.087
25	.032	-.009	.062	-.197	-.048	.082	.053
39	-.019	-.132	.389	.063	.066	.037	.179
41	.108	-.123	.839	.115	-.002	-.013	.744
37	.065	-.121	.290	-.059	-.027	-.011	.107

test .579 and the alphabet test .656. The nature of the material used makes these high correlations intelligible and suggests that these tests cannot properly be used with children. It is not unexpected to find that the Mazes "wrong direction" score has a high negative correlation with intelligence ($-.448$) or that inaccuracy on the track tracer has a somewhat slighter negative correlation ($-.389$). It may be surprising and is certainly interesting that the more intelligent apparently give more truthful self-ratings; the correlation between truthfulness on the lie scale and the intelligence factor is .374.

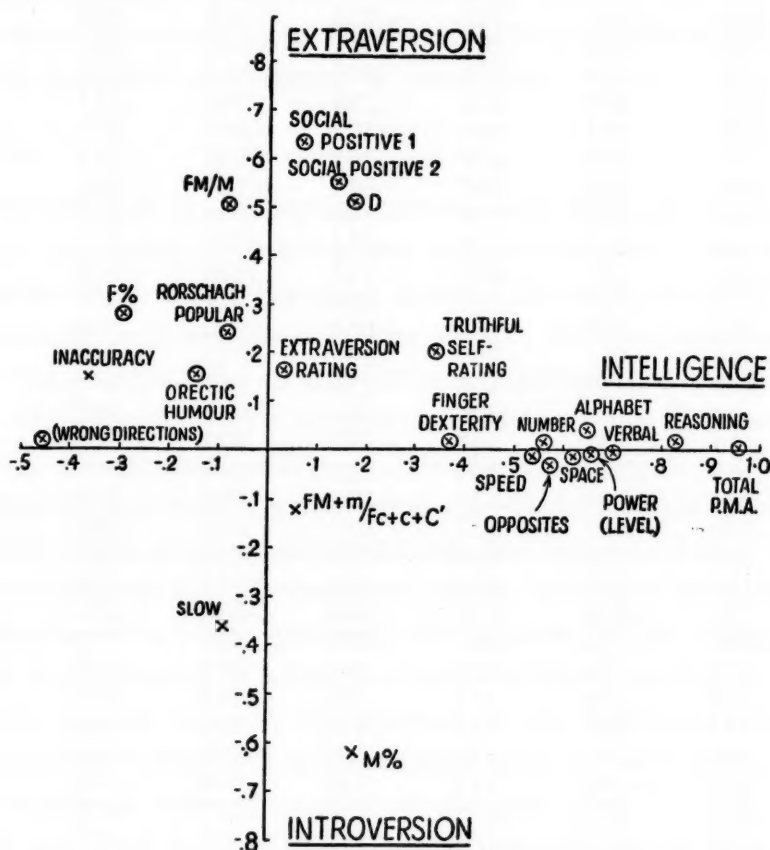


Fig. 1.

An interesting feature of this study is the complete failure of the Rorschach scores to correlate with intelligence. The only one to achieve even the very modest correlation of $-.296$ is the Rorschach $F\%$. This, in spite of the fact that of all the scores included, the $F\%$ score is one of the few that is in general considered *not* to be a measure of intelligence. M , which is usually taken as a good index of intelligence, only achieves a correlation of .191. It is difficult not to conclude that the Rorschach scores which we have used here, and for many of which extravagant

claims have been made as measures of ability, fail to measure intelligence to any significant extent.

We now come to the second factor which has been identified as extraversion. Before discussing this interpretation it will be necessary to present some details regarding the method followed in interpreting the Rorschach scores. While there is a good deal of agreement among Rorschach writers in the interpretation of certain scores, this agreement is far from perfect, and it would be possible in *a posteriori* fashion to explain away discordant findings by referring to some obscure authority as having interpreted this particular score in the manner required to substantiate one's own hypothesis. To avoid this danger, the following method was followed. The scores used were communicated to an expert who had been using the Rorschach clinically and teaching it to students for a number of years. He was requested to write down in detail the relevance of each of the scores to the three variables of intelligence, extraversion-introversion, and neuroticism. He was to base himself entirely on the agreed interpretations of the most widely accepted Rorschach authorities, and on independent factual research evidence. His decisions were written down and implicitly followed in our interpretation; wherever necessary they will be quoted in full. This, of course, does not ensure that other Rorschach experts will necessarily agree; it does ensure that our interpretation of the results is not falsified by an attempt to justify observed findings in the manner outlined at the beginning of this paragraph.

Let us now look at the variables defining the two poles of the factor which we have identified as one of extraversion-introversion. The variable having the highest saturation on the introverted side is *M* % (— .626). According to the expert "a high *M* suggests introversion, a low *M* extraversion". This interpretation has found a good deal of factual support, such as, for instance, a recent study by Barron (1) who has attempted to devise a psychometric measure of *M* by means of a series of specially constructed blots, and who found considerable correlations between movement scores and introverted personality traits. The other introversion score is indicative of slow and accurate work on the track tracer (— .378); this Himmelweit (14) and Eysenck (5) have found indicative of introversion.

On the extraverted side, the two scores having the highest saturations are the two sociometric scores indicative of social popularity and general social liking (.632 and .574). This relationship between extraversion and positive social relationships is, of course, in line with our hypothesis. Only slightly less highly correlated with extraversion is the Rorschach *D* score (.510). This is what our authority has to say about a high *D* score:

"A high D is said to indicate "practical" man, a down to earth extravert; a low D is said to indicate a "theoretical" man, a "theoriser". A high D is associated with hysteria, a low D with dysthymia." The interpretation is thus in accord with our hypothesis. Almost equally high as the D score is the $FM \div M$ score (.501). This score, of course, is not independent of the M score we have already considered, and can therefore not be used to add very much to our interpretation of the latter. However, for what it is worth, our authority summarizes the literature by saying that a high $FM \div M$ ratio "may indicate extraversion", a low $FM \div M$ ratio "may indicate normality, but also introversion and intelligence."

The $F\%$ score has a correlation with the extraversion factor of .286. The interpretation of this score appears excessively difficult. Our authority says that "a high $F\%$ is found in the records of psychopaths"; a high $F\%$ is found in the records of many hysterics ("flat hysterics"). This would suggest that a high $F\%$ is indicative of extraversion. Against this hypothesis speaks the fact that "a high $F\%$ indicates "over-control" which could characterize an introverted neurotic". Altogether, "experts seem in some disagreement" so that we cannot really interpret this particular score. The next Rorschach score, the number of popular replies, has a factor of .242. According to our expert "a large number of popular responses suggests a dull extraverted person or hysteric." Apparently "a small number of popular responses suggests a person out of contact with his environment, or may be due to a perfectionist attitude exhibited by obsessive, compulsive neurotics". In all, he concludes that "a high number of popular responses might, therefore, suggest extraversion, a low number introversion".²

Three more scores are to be considered and lend weight to this interpretation. Inaccurate work on the track tracer has a loading of .162 which, although low, is in the right direction. Orectic humour also has a loading of .162 which is also low, but again in the right direction. Truthful

² Score 37, the Rorschach $FM + m \div Fc + c + C'$ has a loading of -.121 and should therefore be a measure of introversion. According to our authority "high $FM + m$ is probably introverted, high $Fc + c + C'$ probably extraverted by majority opinion". This is in line with our hypothesis, but the correlation is much too small to carry any weight. It may, however, serve to counterbalance item 32, the Rorschach $To \div de$ where "a high score is indicative of an uncritical attitude, perhaps suggesting abnormal extraversion". Here also the correlation (-.189) is too small to carry much weight. Ratios, in view of their well-known statistical unreliability, should never be used in work of this kind, particularly when the scores entering into the ratios are themselves not very reliable (18).

self-ratings, with a loading of .200, is slightly higher and also in line with previous work which has shown a slight tendency for extraverts to obtain more truthful scores on the lie scale. With the possible exception of the *F %* score, we can therefore say that all the scores considered support the interpretation of this factor as one of extraversion-introversion.

A number of items have moderately high correlations with the factor but have not been considered in this connection because they neither argue for nor against our interpretation and may be chance projections on this factor. Among these scores are, for instance, item 44, high finger temperature, which has a correlation of .300, and item 40, high diastolic blood pressure, which has a correlation of $-.230$. Our data are not sufficient to make it possible for us to say whether these additional items, which the reader may like to study intensively in Table 1, throw any additional light on either the identification of the factor or its measurement. The work of Theron and of Van der Merwe (21, 22), as summarized in "The Structure of Human Personality", has opened up the possibility that extraversion-introversion may be related to certain autonomic measures, and certainly this line of enquiry is promising and deserves to be followed up. It cannot, however, be maintained that at the present moment our results throw any further light on this problem.

A few words may be said about the third factor. This, quite clearly, is an autonomic one, having very high saturations indeed on pulse rate resting (.913), pulse rate stressed (.839), systolic and diastolic blood pressure (.389 and .452), and on sub-lingual temperature (.620). Finger temperature is rather out of line ($-.181$), but this may be due to difficulties and inaccuracies of measurement. Dermographic latency has a relatively low loading of $-.216$. The interpretation of this factor as an autonomic one appears somewhat invalidated, however, by the fact that item 2 (sex) has a very high loading of .855. This suggests that quite possibly the correlations observed are produced very largely by sex differences, and are therefore of less interest than they might otherwise be. No further analysis or discussion of this factor will be given here as it does not seem relevant to our main purpose. The same may be said of the remaining three factors, which do not lend themselves to any obvious interpretation and will therefore not be considered any further.

Factor scores were estimated for the first three factors. For the Extraversion-Introversion factors, the following items were used: 53, 54, 31, 33, 28, 4, 6, 46, 52, 36. For the Intelligence factor, the following items were used: 17, 10, 16, 18, 8, 9, 20, 14, 48. For the Autonomic factors, the following items were used: 39, 40, 41, 42, 43, 44, 45. Thus,

each one of our subjects obtained scores on the three factors of intelligence, extraversion, and autonomic activity.

Intercorrelations of factor scores were calculated for fraternal twins and identical twins separately, and are given in Table 2. It will be seen

TABLE II

	Intelligence	Extraversion	Autonomic
Intelligence	—	.030	-.103
Extraversion	.155	—	-.018
Autonomic	-.074	.001	—
Intercorrelations of factor scores for identical twins (below leading diagonal) and for fraternal twins (above leading diagonal).			

that there are no significant relationships between the factors. Next, intra-class correlations were run for the three factors between the identical and also between the fraternal sets of twins. These correlations, as well as the h^2 values calculated from them, are given in Table 3. A test was

TABLE III

	Identical:	Fraternal:	h^2
Intelligence	.820	.376	.712
Extraversion	.499	-.331	(.624)
Autonomic	.929	.718	.748
Intraclass correlations for identical and fraternal twins, on three factor scores.			

made of the significance of the differences between the intraclass correlations. For the intelligence factor, $t = 2.13$; for the extraversion factor, $t = 2.43$; for the autonomic factor, $t = 2.09$. The t values for the intelligence and autonomic factors are significant at the 5 % level; the t value for extraversion is significant at the 2 % level. We may, therefore, conclude with some statistical justification that the differences observed between identical and fraternal twins are unlikely to have been caused by chance factors and would be found again if the study were duplicated. From this it may be concluded that heredity plays a significant part in the causation of all three factors.

One feature in Table 3 requires discussion. It will be seen that the intra-class correlation for the fraternal twins on the extraversion factor has a negative sign. This is an extremely unlikely occurrence on any reasonable hypothesis, but a thorough checking of the figures failed to reveal any errors in calculation. It seems likely that this value represents

a chance deviation from a true correlation of zero, or of some slight positive value, an assumption strengthened by the fact that a correlation of the observed size is not statistically significant. Under the circumstances, however, we cannot regard the h^2 statistic derived for the factor of extraversion as having very much meaning, and it has therefore been put in brackets in Table 3 to indicate its extremely doubtful status. Much more reliance, fortunately, can be placed on the significance of the differences between identical and fraternal twins for this factor which, as has been shown above, is fully significant.

6. SUMMARY AND CONCLUSIONS

In this study an effort has been made to provide evidence for the existence of a factor of extraversion-introversion among children, similar to that found among adults, and to measure this factor. By and large, this attempt has been successful and the factorial analysis reported in this paper gives clear evidence of a strong factor of extraversion-introversion.

It was hypothesized that the concept of extraversion-introversion, as operationally defined in the writer's previous work, would be closely parallel to Rorschach's concept of extratensive-introvertive personality. The inclusion of a number of R scores in the factor analysis made it possible to test this hypothesis, and the results on the whole favoured acceptance of this theory.

Two further factors were isolated in the analysis, namely, one of intelligence and one of autonomic activity. These additional factors were found to be independent of each other and also to be independent of extraversion-introversion. Factor scores were calculated for all three factors for the members of the experimental populations.

As the major aim of the investigation was to study the effects of heredity on extraversion-introversion, the subjects of the investigation were 13 pairs of male identical twins, 13 pairs of female identical twins, 13 pairs of male fraternal twins, and 13 pairs of female fraternal twins. By using standard methods of intra-class correlation for different types of twins, it was shown that [for all three factors, identical twins resembled each other significantly more closely than did fraternal twins]. This was regarded as proof that heredity played an important part in the determination of intelligence, extraversion, and autonomic reactivity.

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DEPTH PERCEPTION OF DOUBLE IMAGES IN THE VICINITY OF OTHER IMAGES

BY

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I. INTRODUCTION

Depth perception of double images was a subject of many discussions and investigations, and also of great controversy. The discussion of this subject was given by the author in his paper "La Localisation en profondeur des images doubles".¹ The object of these investigations was to establish how the double images behave when in the vicinity of other images both stationary and moving.

Four different situations were investigated:—

1. Double images in the vicinity of fixation objects, fixated binocularly.
2. Double images in the vicinity of the images of the other eye.
3. Double images in the vicinity of the images of the same eye.
4. Double images in the vicinity of stereoscopic images formed by fusion of images of two different objects in the field of vision.

In all those situations two alternatives were studied a) when the images were uncrossed images, and b) when they were crossed ones.

Variations were also introduced concerning the position of the double images in relation to other images:—they might have been overlapping, just touching them, or been separated from them by smaller or greater lateral distance.

Further variations consisted of whether both images, of which depth perception was investigated, were stationary, or moving; the last variation was studied in two situations: when the fixation objects or when objects seen in double images were moved.

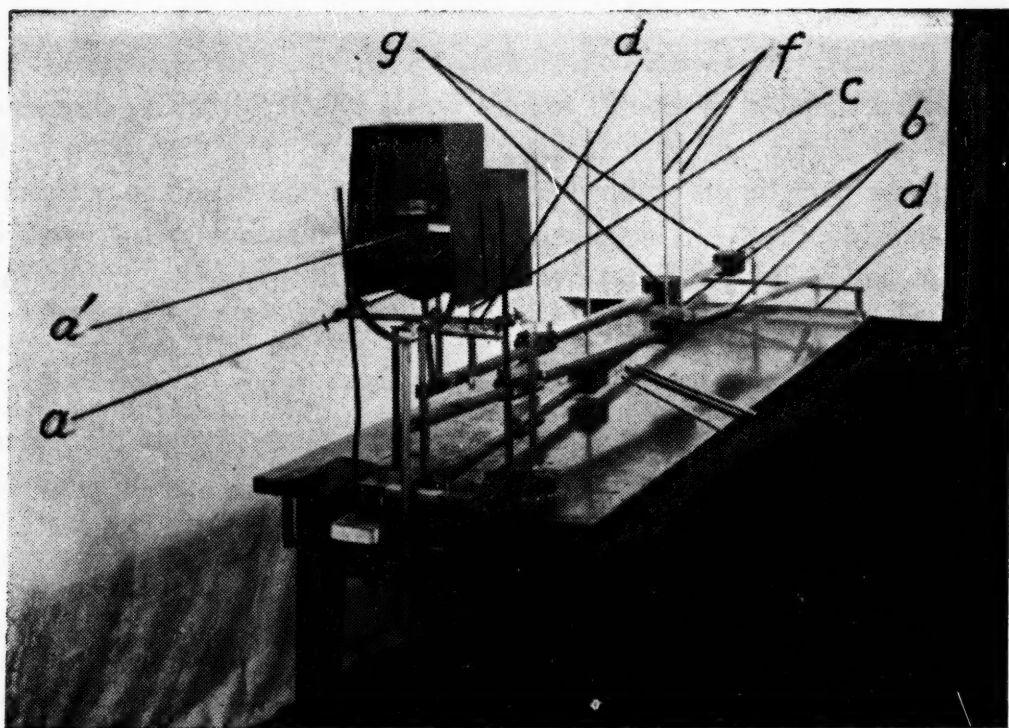
¹ Zajac J. La localisation en profondeur des images doubles, Travaux du Laboratoire de Psychologie Experimental de l'Université de Cracovie, Paris, Félix Alcan 1923.

All this gave a great number of observations which lead to some general principles, formulated below.

II. EXPERIMENTAL ARRANGEMENT

All the experiments were carried out in a room with lighting constant in near field of vision.

The apparatus on which they were conducted was conceived by the author and constructed by Capt. W. Bogucki. The apparatus could be named: *Natural Stereoscope*. The description of this apparatus is as follows (see photograph).



It is composed in the following parts:—

a. A head-rest with a slit (a') through which the observer viewed the objects in the field of vision.

b. An apparatus in form of three bars on which were placed objects in form of rods in the field of vision. These rods could be moved in any direction and their position measured in terms of distance from the eye and angle from the line of sight.

c. An apparatus on which the glass-plates, or colour filters could be placed and turned about a vertical axis.

d. An angular scale for measuring the angles of turning of the glass-plates and colour filters.

- e. A screen, limiting the field of vision.
- f. Rods of different diameters² and colours.

The head-rest served to place and fix the head of the observer and to give him required vision. The slit could be enlarged and reduced as required in vertical and horizontal directions. The whole of this viewing apparatus was limited by black screens in order to eliminate the distraction of the observer by other objects and light effects.

The apparatus for placing and moving objects consisted of three steel bars 5 ft. 8 inches in length, with a millimeter scale on each of them. All three were anchored on the same line parallel to the line uniting the centres of eyes of the observer. The middle bar represented the base line of the so-called median plane in the field of vision. This bar was fixed on the highest level. Two others were so set out on different levels beneath the middle one, that their two near ends could be fixed at the interocular distance of any observer. These points could be moved in the horizontal direction and fixed, when the point corresponded to the centre of the eye. The fixing points served as axes around which they could be turned for about 18 deg. On these bars, slides (g)—three on the middle one and two on the other two—were placed on which the rods were placed vertically and moved as required. The rods could also be rotated: for this purpose they were placed on rotating supports (h) put on slides (g). The rods of different thicknesses and different colours served as objects in these investigations and demonstrations. Other objects could also be used for investigations on this apparatus.

The apparatus enabled one to investigate depth perception and other phenomena in various combinations of geometrical distance and visual angles of the rods both at rest and in motion.

The apparatus enabled one to place in front of both eyes glassplates or colour filters and to turn them to the right or to the left around vertical axis, and every turn could be measured by means of an angular scale placed beneath. The holders of the glass plates could also be lowered or raised so that the whole slit, or only part of it, could be covered by them. In addition the lateral holders enabled one to place two different glass-plate or colour filters one above the other in front of one eye so that the effects of turning of two different glass-plates through the same angle could be studied.

The angular scale (d) placed at the far end near the back screen enabled

² The diameters of the rods used were: 8 mm, 6 mm and 3 mm. They are referred to below as thick, middle sized and thin rods respectively.

one to read the angles between the actual position of the bars and their positions when they were parallel to the middle bar, which represented the median plane.

The screen (e) made of white cardboard limited the field of vision and was placed at a distance of about 6 feet.

The whole set-up gives many possibilities for investigation and demonstration of stereoscopic and other phenomena in near field of vision in a natural setting; it enables one to investigate the relation of those phenomena to the geometrical settings, it gives the opportunity not only to study the changes in depth perception, but also in relief of objects and stereoscopic images, and gives further the opportunity to study those changes when the objects are moving. It enables one also to establish relations between those phenomena and convergence, accommodation and pupillary changes.

The apparatus enables one to study and compare the phenomena occurring in monocular vision with those in binocular vision with double images and in stereoscopic vision; this is not possible with normal stereoscopes.

Lastly other phenomena such as binocular colour mixture and colour-rivalry can be here investigated in a new experimental setting.

III. DESCRIPTION OF EXPERIMENTS AND THEIR RESULTS

a) The first series of experiments consisted of placing two middle size gray rods at a distance of about 30 cm from the observer and about 3 cm of lateral distance and a thick rod placed behind, so that when one of the nearer rods was fixated the farther rod was seen in double images. The lateral distance of those double images was dependent on the distance of the farther rod from the observer and from the nearer rods. One of the nearer rods served as a fixation rod.

The geometrical distance of the farther rod can be so adjusted that the two double images just touch the inner parts of the two nearer fixation rods, i.e. the right image touches the left side of the right rod and the left image touches the right side of the left rod. When from that position we move the farther rod away from the observer, its double images first overlap with the nearer rods; when it is moved still farther away the double images appear on the outer sides of the nearer rods, overlapping at first, then touching them, and at last losing contact with them. When from the initial position the farther rod is moved towards the observer, the double images lose their contact with both nearer rods and the lateral distance

between them and the nearer rods grow at the same time as the double images approach each other.

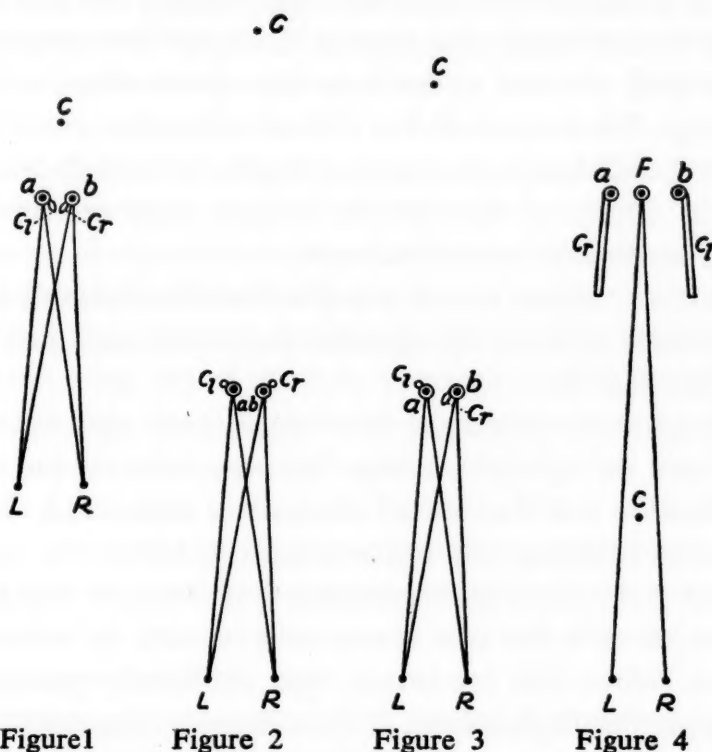


Figure 1

Figure 2

Figure 3

Figure 4

The investigations had as object to study the depth perception of the double images in these various situations.

In the initial situation where the double images touched the inner sides of the nearer rods, these images were seen several times protruding in form of "Walls" or "Ribbons" in front of the nearer rods with their inner sides nearest and their sides touching the fixation rods at the distance of the latter ones (See Fig. 1). This effect was observed by 6 out of 8 subjects. Two subjects saw in this position the red double images at the same distance as the fixation rod. Six persons also saw them in the shape of "Ribbons" or "Walls" and two only saw them as rods.³

³ *Lettering of figures:* In all figures black dots represent the positions of the eyes of the observer and the *geometrical location of rods of every colour*, and circles, parallelograms and ellipses represent, what *we see* when fixating the fixation rod F. Parallelograms and ellipses represent the changed, elongated forms ("Walls", "Ribbons", "Strips", etc.) of double images seen in the vicinity of fixated objects, stereoscopic images or other double images. Black rods surrounded by circles denote rods as located geometrically and at the same time as seen, when F. is fixated (The dots and circles denote only location, and not geometrical or seen size of the rods). The meaning of letters: L, R,—left, right eye, F—fixation rod, a, b, c—geometrical position of rods, a_l , b_l , c_l —double images of a, b, c, belon-

If now the double images were overlapping with the two nearer rods and the rod seen in double image was turned slowly to the right, the right image was seen taking cover behind the right fixation rod and at the same time receding; the left image was seen at the same time coming out from behind the left gray rod and at the same time protruding further in front with its inner edge. The same effect, but in reserve direction, could be observed when the head of the observer was turned slightly to the left. These changes in size and in depth of the double images were continuous if the corresponding movements were continuous.

When the farther rod was moved to a position where double images were touching the nearer rods on the outside, they were seen just behind the nearer rods (See Fig. 2).

If from the position where the two nearer rods and the two double images were seen as equidistant from the eyes, one of the nearer rods serving as a fixation rod was moved towards or away from the observer, the double image touching this rod was seen to follow the movement of the fixation rod and by this movement the differences in depth perception were observed between the two nearer rods as well as between the two double images. When this movement was sufficiently pronounced, one observed not only the displacement of the corresponding double image, but also a change in its thickness; during the movement towards the observer, the thickness of the double image diminished and it grew during the movement away from the observer. These changes of perceived thicknesses of the double images could be verified when changing the fixation from one of the nearer rods to the other. No such movements of the double images were observed when the stationary nearer rod was fixated and the other rod was moved.

When the position of the farther rod was chosen so that the right double image was seen touching the inner left side of the right fixation rod and the left one touching the outer left side of the left nearer rod, in that case the right double image was seen mostly in the form of a "Wall" or "Ribbon" protruding in front of the right fixation rod, and the left image was seen behind the left nearer rod, mostly in the form of a rod, but sometimes also

ging to the left eye, a_r, b_r, c_r ,—double images of a, b, c , belonging to the right eye, $S(a, b_l)$ —stereoscopic image produced by fusion of the image of a by the right eye and of the image of b by the left eye; N-nearer, F-farther, E-equidistant as compared with the other double image. In Figures 11-22 neither observers eyes, nor geometrical positions of the rod c are represented, only rods a and b , and double images of c . Experimental arrangement for situations shown in those figures was similar as that in Figs 1, 2, and 3.

in the form of a wall receding from the observer with its outer edge (See Fig. 3). These differences in depth of the double images could be eliminated by moving the left nearer rod towards the observer, or the right rod away from the observer. It was found for example that for the distance of 30 cm of the nearer rods and the lateral distance between them of about 3 cm it was necessary to move the left rod for about 2.5–4 cm towards the observer to see the two double images at equal distance, although the shape of the two images and their thickness was not the same.

Other series of experiments were carried out with double images, which were not in immediate touch with the nearer rods.

First of all a situation was chosen where the corresponding double image was separated from one of the rods by much smaller lateral distance than from the other rod. The right double image was seen to the left of the right nearer rod separated by a small space, and the left double image was touching the left nearer rod on the outside. When in that case the right nearer rod was moved towards the observer the right double image moved with it; not only that; when the movement towards the observer was more pronounced the left nearer rod was seen also in double images and its right member also moved with the right nearer rod which was fixated. The other image of the nearer rod together with the left double image of the farther rod touching each other remained stationary.

b) Similar phenomena but in inverse direction were observed when two rods, one of which served as a fixation rod, were placed farther away and the nearer rod was seen in double images.

Two red thick rods were placed symmetrically to the median plane at a distance of about 130 cm with lateral distance of 7–8 cm. Between them a thin yellow rod served in many cases as fixation rod. A gray rod of middle size was placed so as to form double images in the neighbourhood of those two red rods. When these images were seen in the neighbourhood of the farther rods, one of which was fixated, the following observations were made: If the double images were touching the red farther rods on their outer sides, they formed very distinct and very wide "Walls" protruding towards the observer (See Fig. 4).

In this case the double images were crossed ones and in the situation mentioned above were formed on temporal sides on retinas of the corresponding images of farther rods, and then they were seen nearer. When the nearer rod was pushed to the position where the double images were seen touching the farther rods at their inner sides, they were seen farther away extending in form of "Walls" behind the farther red rods (See Fig. 7).

If the direction of vision of two double images is so arranged that both appear on the left of the two farther rods, the left one extends in the form of a "Wall" towards the observer and the right one away from the observer

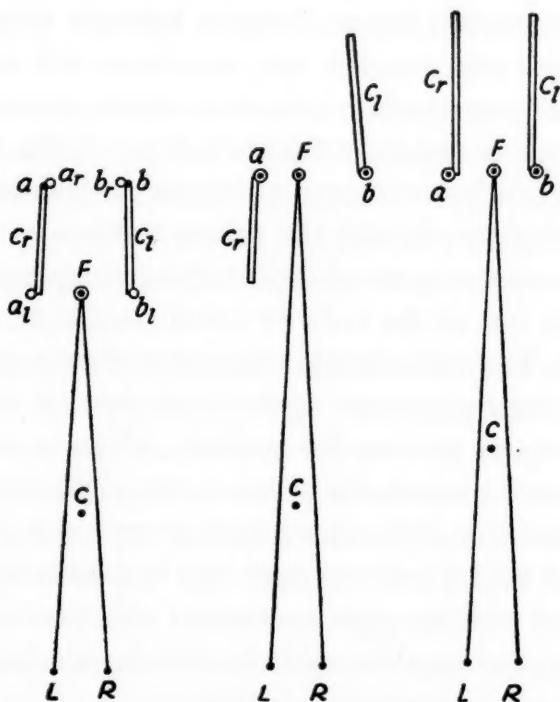


Figure 5

Figure 6

Figure 7

(See Fig. 6). The farther rods do not change their distance during the movement of the nearer rod provided they are not covered totally for one eye by the image of the nearer rod.

When in one of the positions described above the farther fixation rod was moved towards or away from the observer, the double image touching it was seen moving together with it.

In the case where one double image of the nearer rod covers totally one of the farther rods for one eye, we have to do not with a double image and a binocular single image but with two double images of two different objects seen by each eye separately. In that case they behave like two double images of two different objects fusing into one stereoscopic image. When the nearer rod is seen at greater visual angle than the farther rod for the other eye, then in case, where the inner edge is formed by the double image of the nearer rod, the whole stereoscopic image is seen farther away, when it forms the outer edge it is seen nearer than the fixation rod. When the nearer rod is moved towards the observer, the whole stereoscopic image comes nearer to the observer, when it is moved backwards, the whole

image recedes. The same effect can be obtained if instead of moving the nearer rod towards the observer and away from him the rod is moved to the left and to the right. In the first case the stereoscopic image recedes from the observer and in the second it moves towards him. In several cases we observed also another phenomenon. If within the stereoscopic image the red image could be perceived at the same time as the gray one, both of the above described kinds of movement of the nearer rod provoked an impression that the red rod was dragged by the gray one in front or behind the middle yellow fixation rod.

Other phenomena occurred, when the farther, red rod was covered only partially by the nearer, gray one. In that case the images of the red rod by the two eyes were of different sizes. Then, as we know from the investigation on "Walls" phenomena, a "Wall" is formed of the red images within the whole image formed by fusion of images of gray and red rods. When the inner part of the red rod was covered by the right image of the gray rod, the red wall was protruding towards the observer with its inner part, the outer part remained anchored at the same distance as the fixation yellow rod. When the outer part of the red rod was covered by the image of the gray rod, the inner part of the red "Wall" was anchored at the distance of the fixation rod, but its outer part was seen receding behind it.

The above described experiments dealt with the double images in contact with binocular single images of rods, which were fixated or which were situated in the same horopter with the fixated rod.

To illustrate the width of the "Walls" formed by double images in front or behind the fixation rod, the following example is given:—

The two thick red rods were placed at a distance of 130 cm with 11.2 cm of lateral distance between them. A thin yellow rod placed between them at the same distance served as fixation rod. When a gray, middle-sized rod was placed at 42.25 cm then its double images were seen touching the red rods at the outside and protruding towards the observer in the form of "Walls" or "Ribbons" (Fig. 4). To measure their width we bring the yellow rod nearer towards the observer so that it is seen at the same distance as the nearer edges of the "Walls". But as we move the fixation rod nearer to the observer the whole picture becomes somewhat complicated, because in this case the red rods are seen also in double images and these two images of both red rods are seen at both edges of the gray double images ("Walls"), one on the outside and at the same distance as the fixation rod, and the other at the inside and at the farther edge of the gray image (Fig. 5), in between the gray image in the form of a "Wall" or "Ribbon". The yellow fixation rod must come at a distance of 104 cm

from the observer, i.e. the width of the whole image corresponds to the geometrical difference of 24 cm.

To see the images of the gray rod touching the red rods at the inside and to form "Walls" behind the red rods, one must move the gray rod to the position of 53.5 cm (Fig. 7) from the observer and the yellow fixation rod must be moved to the position at 171 cm from the observer to obtain its equidistance with the farther edges of the gray walls (Fig. 8). In this case the gray wall is as before limited by two double images of the red rod and the width of the whole image corresponds to the geometrical difference of 41 cm.



Figure 8

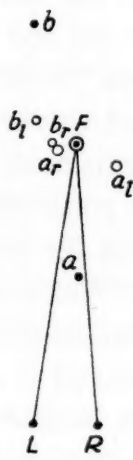


Figure 9

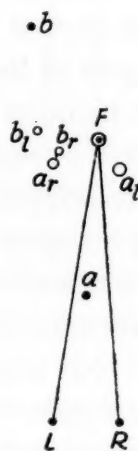


Figure 10

c) Now the question arises how the double images behaved in the neighbourhood of other double images.

Let us take first the question of the double images belonging to different eyes. For this purpose the arrangement is adopted similar to that in the case of the "Walls" phenomena; the thin fixation yellow rod is placed in the middle, the thick red rod is placed on the middle bar at a distance of about 125 cm. The nearest thin gray rod is placed on a rotating support. All three rods are placed on the median plane. When the right image of the nearer rod (belonging to the left eye) comes into contact with the farther image of the farther red rod (belonging to the right eye), the

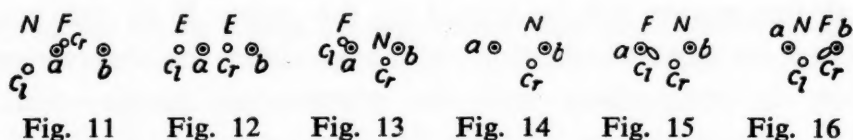
following phenomena can be observed: There is a position of the nearer rod where the stereoscopic image resulting from its fusion with the red image is seen at the same distance, as the fixation rod. If starting from that position we move the nearer rod towards the observer, the stereoscopic image comes in front of the fixation rod. It looks as if the moving gray rod was dragging the red image with it. The same effect is seen when the rod is turned to the left. When the nearer rod is moved away from the observer the stereoscopic image recedes behind the fixation rod; the same thing occurs if the nearer rod is turned to the right. If in the position of equidistance we move the fixation rod it approaches the observer or recedes from him in comparison with the stereoscopic image—which looks stationary—depending on whether the fixation rod is displaced towards the observer or away from him. The amplitude of these movements is limited to the positions where the two double images fuse into one single stereoscopic image. Beyond those positions they are seen separately.

d) Quite a different picture is presented by the case where two double images of the same eye touch or overlap each other. To study that, we turn the nearer rod to the right so that now the left image of the gray nearer rod (belonging to the right eye) comes in the neighbourhood of the right image of the farther red rod. Now, if the red rod is not completely covered by the gray image, we see a composite gray-red image which looks to be nearer than the fixation rod. Its red part looks to be attracted by the gray one but the gray part looks always nearer than the red one. When the gray image is overlapping over the inner part (nearer laterally to the fixation point) of the red rod, the red part of the image looks at the same distance as the fixation rod, or a bit farther away; the gray part is seen always nearer than the fixation rod (Figs. 9 & 10).

If in this situation the fixation rod is moved towards the observer or away from him, the composite image is seen moving with it, contrary to what is observed in the above described case of the stereoscopic image formed by two images belonging to two different eyes. When now the nearer rod is moved towards the observer, or away from him, without changing its direction of vision, the composite image does not move at all, only the right double image of the gray nearer rod moves towards the observer or away from him, depending on the direction of movement of the rod; at the same time the lateral distance of this latter image from the composite image increases or diminishes according to the direction of movement of the rod.

e) Another series of experiments consisted on investigating the relation of depth perception of double images with their lateral position vis-à-vis

the fixation objects. This lateral position was changed by turning the object in double images. The arrangement for these experiments was the same as in the first series, i.e. two nearer gray middle sized rods placed at a distance of 30 cm, one of which was fixated and a thick red rod placed behind them and seen in double images. The perception of depth of double images in different positions is given below and illustrated by figures. In the figures full black dots surrounded by circles represent the gray nearer



rods, and the other circles the double images of the red rod. The letter *N* denotes the perceived nearer distance, the letter *F* the farther distance, and *E* the equidistance as compared with the perceived distance of the other double image. So when the right double image is touching on the right the left gray fixated rod, it is seen farther away than the left double image which at the same time is generally seen nearer than the fixation rod (Fig. 11). When the double images are placed symmetrically on both sides of the left fixation gray rod, they are seen equidistant with one another and also generally at the same distance as the fixation rod (Fig. 12). When the position of the double images is as shown in Fig. 13, the right image is seen nearer than the left one and at the same time nearer than the fixation rod. The other image is seen also farther than the fixation rod. From Fig. 14 we see that when the left double image is covered by the left gray rod, the right one is seen in front of the fixation rod. In the position illustrated by Fig. 15, when the left is fixated, the right image is seen still nearer than the left one, but both are seen generally in front of both gray rods. When the right gray rod is fixated, not so consistent observations could be made. Fig. 16 represents the opposite situation and here also the depth perception is the reverse of that in Fig. 15, but here the right gray rod must be fixated. Fig. 17 is the opposite of Fig. 14; Fig. 18 the reverse of Fig. 13; Fig. 19 the opposite of Fig. 12, and Fig. 20 the reverse of Fig. 11. Fig. 21 represents the situation where the left double image touching on the right the left gray rod and the right double image touching the right side of the right gray rod are seen; the left one nearer than the right double image and at the same time in front of the gray rods, while the right double image is seen farther away than the gray rods. From Fig. 22 we see that when the right image is partly covered by the near gray rod, and the left double image is

touching the left gray rod on the right, the last one is seen nearer than the right one.

The continuous sideways movements of the farther rod resulted in continuous changes in perceived depth of the double images; so, for example, when the red farther rod was moved sideways from right to left, the double images passed from the position shown in Fig. 20 to the positions in Fig. 19, and then in Fig. 18; at the same time they changed their depth perception—the right double image receded and the left double image came nearer to the observer. In the case of the left double image several subjects observed the movement from behind the right fixation rod in front of it, along a diagonal plane. Other persons observed abrupt changes when the double images in this sideways movement came to the touching positions with the gray fixation rods.

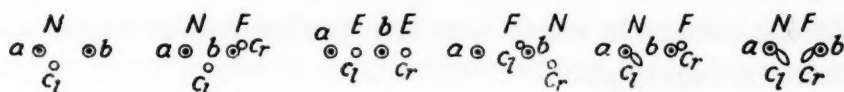


Fig. 17 Fig. 18 Fig. 19 Fig. 20 Fig. 21 Fig. 22

Some other observations were also made. One subject, for example, has seen both images equidistant, in the position illustrated by Fig. 20; whilst in the symmetrical position shown in Fig. 11, his observations were the same as those of other subjects.

When the double images were in touching positions which corresponded to the temporal disparity in comparison with the fixation rods, i.e. the right image was touching the right fixation rod on the left, or the left double image touching the left one on the right, then generally they were seen in the form of flat or ellipsoid images protruding in front of the gray fixation rod. One subject has seen in these circumstances the red double images at the same distance as the gray rods, and very rarely in front of them, but he contended that he never has seen in stereoscopic films the effects of objects thrown out of the screen on the spectators, although most persons were surprised by that effect. Another person also saw this very rarely, but she was very inconsistent in her observations. She saw mostly in those cases the red double image touching the fixation gray rod rather "whitish" than red, which was the indication that she had not properly fixated the gray rod but rather the stereoscopic image formed from the red double image, and one of the double images of the gray rod, derived from the inappropriate fixation.

f) Another series of observations was made to verify how the double images behave when touching not the fixation rod or a rod placed in the

horopter with it, but a stereoscopic image formed by fusion of two different rods and to compare this with the case of the double images being in touch with the fixation objects. For this purpose the following arrangement was adopted:

Two thickest red rods (*a* and *b* in Figs. 23—26) were placed farthest away at a distance of 105 cm; 4.3 cm of lateral distance between them. The middle fixation rod was placed at a distance of 70 cm. In these positions of the rods, when the gray rod (*F*) was fixated, a stereoscopic image (*S*) of the farther two was seen at the same distance from the observer as the fixation rod. Now another thin gray rod (*c* in Figs. 23—26) was placed on the middle bar on a rotating support. The geometrical distance of it must be so chosen that the double images must be clearly seen. (For the author the distance was 46.5 cm.) This rod was seen in crossed double images, when the middle one was fixated. It was turned so as to bring one or the other double images in touch with the fixation rod or stereoscopic image on their left or right side.

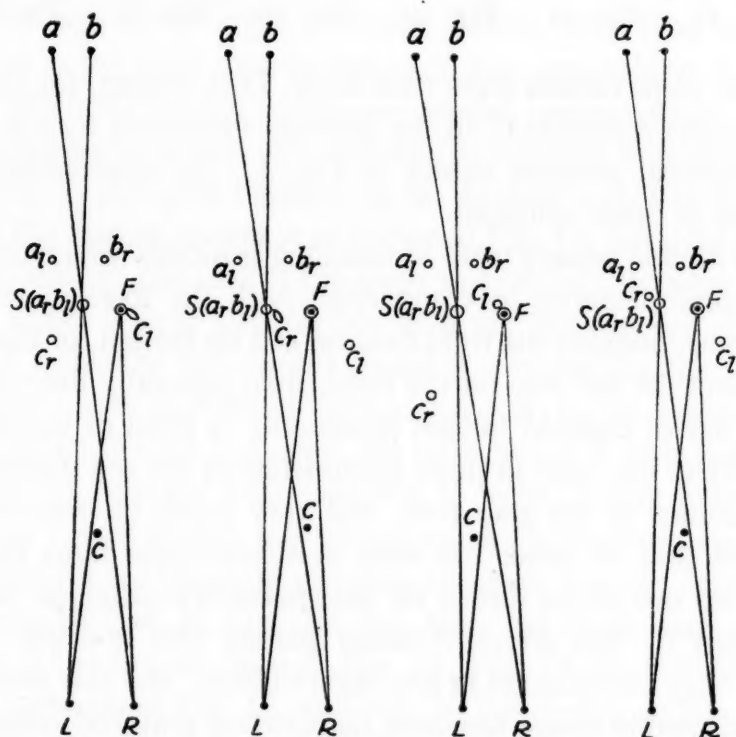


Figure 23

Figure 24

Figure 25

Figure 26

When now the right double image (belonging to the left eye) was in touch with the right side of the fixation rod (Fig. 23), it was seen in the shape of a small "Wall" protruding towards the observer with its outer part, which could be seen when the perceived depth of the image was

compared with the distance of the stereoscopic image. When the right double image was turned so as to come in touch with the right side of the stereoscopic image, quite similar image was seen (Fig. 24). When the same double image came in touch with the left side of the fixation rod or the stereoscopic image, the double image was seen behind the two, mostly in the form of a rod, but sometimes in the shape of a "Ribbon" or "Wall" receding with its outer part away from the observer (Figs. 25, 26). When the left image was brought into touch with the fixation rod or the stereoscopic image inverse phenomena occurred.

These observations prove that double images behave in the same way when touching stereoscopic images, as when they are touching the fixation rods.

It must be underlined that with the arrangement described above all the observations described in this paper could be made. Other arrangements were adopted to simplify the observations. The last one is too complicated for an observer unaccustomed to the observations in such arrangement.

IV. DISCUSSION OF THE RESULTS

a) The place in the third dimension of the double images is dependent on very many factors. The influence of some of them were investigated here, when the observations were made with double images placed in the neighbourhood of other images.

The factors investigated here were:—

1. The actual state of fixation.
2. The part of the retina involved (nasal or temporal).
3. The geometrical distances of the objects seen in double images and of fixation objects.
4. The movement of different objects in the field of vision.

1. The actual state of binocular fixation is responsible for the place on retinas where images of different other objects are formed, among others for the binocular disparity of those images and also for whether the objects are seen in double images, or whether they fuse in stereoscopic images, etc. I do not mention here such features as size, distinctness, brightness, saturation of colour, etc. The fixation object is, so to speak, anchored in space, although very often we cannot estimate how far it is from the observer. Other images—stereoscopic, double or monocular—have their localisation in the third dimension in comparison with the binocular fixation object. So the fixation object represents a

stabilising element vis-à-vis the others. The fixation may also be made on stereoscopic images, and then we can compare the distance of the object which served previously as a fixation point with that of the stereoscopic image. The conception of horopter is valuable not only for objects seen singly in the field of vision, but also for all stereoscopic images obtained by fusion of images of two similar objects. The images of fixated objects are formed on central parts of foveas.

2. The second important factor is the place on the retina, on which the double images are formed. As has been confirmed in many experiments in stereoscopic vision, the nasal disparity yields the greater distance, while the temporal disparity gives nearer localisation as compared with fixation objects and other stereoscopic images. In these experiments the same was confirmed as concerned the double images being in touch or in the vicinity of binocular fixation objects or stereoscopic images, or other double images but belonging to the other eye. It was found further that these rules do not apply to the images of the same eye. Nasal or temporal place on the retina of one eye do not yield different location in the third dimension as in the case of images belonging to two eyes. This leads us again to the contention that the monocular depth perceived in near field of vision is based on quite different factors than the depth perception of stereoscopic images and double images if they are seen in the neighbourhood of objects which are fixated binocularly, or of stereoscopic images and also of double images belonging to the other eye. So the Hering's theory is confirmed here only partly, although he also observed sometimes phenomena of different location in the third dimension of two members of double images especially of objects which yielded different lateral distances of double images from the fixation object.

3. The relation between the geometrical distance of objects seen in double images which are located in the lateral neighbourhood of fixation objects is very significant for the whole problem of depth perception. When our convergence is fixed and determined by fixating an object in the field of vision, the geometrical distance of the object seen in double images influences the shape of the double image. According to the state of accommodation of the corresponding eye the double image is seen larger or smaller, more or less distinct and in greater or smaller lateral distance from the observer. Accommodation also determines in some way the depth perception of the monocular image, but only when it is not in close neighbourhood with the fixation object or other double image. As all

features of the double images are related to the geometrical distance of the object, the geometrical distance can also be responsible for their depth perception. But when the double image comes in the vicinity of a fixation object many of the features mentioned above are still related to the geometrical location of the objects seen in double images, but their location in third dimension is no more dependent on geometrical distance. They always are seen nearer or farther according to whether their disparity with other images is temporal or nasal, and irrespective of the geometrical position of the object as compared with other images. When in touch with them they may assume a shape of a "Wall" or "Ribbon". They may also in this case change saturation of the colour and their thickness and brightness. The only condition of all this is that the images concerned must not differ much in distinctness with objects or images in the neighbourhood of which they are seen.

So we may conclude that in near field of vision depth perception of the double images in the vicinity of fixation objects, of stereoscopic images and of double images of the other eye is subject to the laws of binocular disparity. It is otherwise in the case of double images seen in the neighbourhood of double images belonging to the same eye. In this case both are seen close together independently of the difference of geometrical distances of corresponding objects; it looks as if the image of the nearer object receded and the image of the farther object came nearer to form one composite image, but always the part of it belonging to the nearer object is seen nearer, and the part belonging to the farther one farther away, this time independently of what kind of difference in retinal position (nasal or temporal) is involved.

About the position of this composite image in comparison with the fixation object one can say that its middle part is seen approximately at the same distance as the fixation object, although in more but not too distant lateral situations the whole composite image may even come in front of the fixation object.

4. The introduction of the element of movement into the field of vision underlines and emphasises the rules discussed above. Two kinds of movement were investigated; one of fixated objects and the other of objects seen in double images. When a fixation object being in touch with a double image is moved, the double image moves with it, the other image belonging to the same object is seen diminishing or increasing its lateral distance and also its distance in third dimension in comparison with the fixation object according to the direction of movement of the fixation rod. When the

object seen in double images is moved, the double image being in touch with the fixation object does not move at all being perceived as attached to the fixation object, while the other image is seen moving laterally and in the third dimension.

When the double image is in touch with a double image of another object belonging to the other eye, then the movement of the fixation rod in the third dimension is seen as movement forward or backwards, while the stereoscopic image formed by those two images remains stationary. When on the contrary one of the double images is moved forward or backward, or sideways this brings the changes in third dimension of the stereoscopic image as compared with the distance of the fixation rod which in this case remains stationary.

When a double image is in touch with a double image of another object for the same eye and in near lateral distance from the fixation rod, then the change in distance of the fixation rod brings with it the change in distance of the composite image which moves with the fixation rod. If now the object seen in double image is moved forwards or backwards, remaining in the same lateral position with another image, no change is seen in the position of the whole composite image nor of its parts, but the other double image belonging to the moved object changes its lateral position as well as its location in the third dimension.

b) In addition to the depth phenomena occurring when double images were seen touching other images in the field of vision, the effects of changing shapes, inclination to the median plane and changing width of these images were observed. The images of objects in the shape of rods assume frequently the form of "Walls", or "Ribbons" when touching fixation objects, stereoscopic images, or images belonging to the other eye; not so when they touch images belonging to the same eye. They assume an elongated form of considerable width and are inclined to the median plane etc. This effect of "relief" is here a feature of double images, which are in touch with images of the other eye or with objects fixated binocularly. It is to be noted that fixated objects do not change their shape similarly, neither do the double images belonging to the other eye, the latter ones, if they present to the observer smaller angular size. In previous investigations one made a very striking observation: when a thicker image appeared brighter and a thinner image dark and one moved the eyes along the "Walls", the following phenomenon occurred: when the eyes were directed to the back of the "Wall", the thin dark line of the farthest rod was seen at the inner part of the "Wall", and behind the fixation point.

When one moved the eyes along the "Wall" towards its nearer edge, the thin distinct image of the farthest rod was displaced to the outer edge of the wall, and at the same time it was seen as if moved to a position nearer than the fixation point. One must assume that in the first case the convergence and accommodation of the eyes were made behind the fixation rod, and in front of it in the latter case. It would seem that no accurate convergence nor accommodation occurred at the fixation point, nevertheless, the difference in distance was seen quite definitely.

This may lead to the assumption that when stereoscopic fusion occurs and the effect of "Wall" or of slanting surfaces is perceived, then one image only (the larger one as measured by angular size) changes its appearance, inclination to the median plane and width, the other one remains as it is seen monocularly; but the condition of seeing the double image in relief mentioned above is that it must overlap, touch the other image, or be seen in its vicinity; this seems to be one of the forms of "reactions" of objects in the field of vision of one eye on perceived forms of other objects in the field of vision of the other eye. This is one of the forms of structuring binocular perception; it needs and deserves more detailed investigation.

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BELONGINGNESS AS A UNIFYING CONCEPT IN PERSONALITY INVESTIGATION

BY

LÉON LITWINSKI

1. INTRODUCTION

According to W. B. Pillsbury (7) "the tendency is for the number of different concepts to be reduced rather than increased."

In philosophy, Henri Bergson however dreaded a conceptual sclerosis. Emile Boutroux, for his part, desired as many concepts as possible, taking them to be the royal road of research.

We must keep in mind that scientific progress is to be found not only in unification but in differentiation.

In psychology, Stagner and Karwoski (8) insist on the function of unifying concepts not merely in the region of homeostasis, but also in contextual cross-references, on the aspect of the convergency of different concepts, since all that rises converges. Contemporary psychology seeks for frames of references for interpreting and interrelating data from many diverse fields of personality investigation.

Reference to facts or the invocation of metaphors are only of temporary or relative importance in the progress of science.

When a science possesses an abundance or an excess of facts, it is obliged to seek for a sound theory capable of introducing a little order. It clings to this theory as to a life-buoy. "Doing something without a theory, rightly observes Dallenbach (2), is not a scientific experiment. It is mere busy work. It has no scientific purpose nor meaning."

J. J. Thomson, the discoverer of the electron, represented the distribution of negative electrons in a diffused positive electric field, by the image of a plum pudding in which the currants are embedded in the dough. The genius of Rutherford replaced this metaphor by that of a planetary system, in which the electrons turn around a positive nucleus.

2. VARIOUS FORMS OF BELONGINGNESS

In the psychology of belongingness, in the study of personality integrated in the structure of a whole, we no longer start from the standpoint of a simple bilateral relationship, but try to conceive of systems of a dimen-

sional nature. Although deeply mystical, Charles Péguy distinguished in the human character ties that were purely horizontal and others that were vertical, that is to say, in depth.

We should avoid over hasty measurement of things which still lack conceptual clarity. Psychological thinking gains enormously from being shaded, differentiated. Thus the unifying concept of belongingness is distinct from, although connected with, that underlying various forms of integration or identification, such as possession, affiliation, submission, commitment, participation, fidelity and loyalty. As for disintegration, this appears in manifold forms, such as abandonment, renunciation, abjuration, separation, divorce, treason, repudiation, inconstancy, deviation, flight and so on.

To commit oneself and to subject oneself, for instance, are in no sense synonyms, but rather antonyms, like liberty and slavery. The slave subjects himself through his need for security. The intervention of the sense of security creates a feeling of belongingness which satisfies him. Thus, in the eyes of the serf, to belong to his lord seems of greater value than to be independent. This explains why the serfs attached to the estate of Turgenev the novelist begged their master not to set them free. It was the fear of the unknown, of a separate existence, which led the peasants in question to oppose the humanitarian offer of emancipation when serfdom was abolished in 1861.

There are other forms of slavery than the relationship of slave to master. One becomes the slave of one's own wealth, of one's passions, of obsessions and doctrines. The concepts of the miser, the tyrant, the diehard, the obsessive, are related to belongingness, and constitute so many demons or enemies of the self.

The feeling of the need for belongingness is present in the infantile mentality, as I have already shown (4). It is equally present in the feminine mentality. The woman is mainly obedient to an inner wish for belongingness, to be protected. In the belongingness woman finds both the satisfaction of her highest ambition as a lover and the constancy of the tie which gives her security.¹

3. ORGANISM AND ENVIRONMENT

Amongst the various functions of the self, that of belongingness allows the individual to distinguish between inner or subjective reality and outer

¹ In *Les Fourberies*, Christian de Scapin says: "Les femmes préfèrent les hommes qui les prennent sans les comprendre, aux hommes qui les comprennent sans les prendre."

or objective reality. This sense does not however appear at birth but is gradually learned. When organised, it puts an end to aduality, which implies a confusion between the self and the non-self, the subjective and the objective, the primitive lack of differentiation. This confusion between "mine" and "thine" persists in the early life of every child.

Although apparently simple, the distinction poses difficult problems, when one tries to set limits, to establish the frontier between the two realities in question and to localise them. In the pathology of personality and the consciousness of the self, the asomatognostics, i.e. those who are unable to apprehend the existence of part of their body, show an interaction between a disordered awareness of the body and the phenomena of depersonalisation. They become such the moment that consciousness is deprived of the special tonus of "mine", that is to say, of belongingness. In this case, objects appear to us as strange, presenting themselves mechanically.

Some maintain that this frontier in question is constituted by the skin, an excessive concretisation, which might incur, in psychology, the charge of reification; whereas others conceive it *in abstracto*, in the shape of a line of reference localised in the head, in the region of the eyes. It constitutes, as it were, a junction between right and left, back and front, symbolising the past and the future.

Andreas Angyal (1) finds it childish, almost laughable, to conceive the skin as a kind of fixed frontier which separates the organism from its ambience. For him, organism and environment are not separable as structures in space. The bodily surface is not the boundary of the organism. The fact of having ingested food, of having located it in some internal cavity of the body, called the stomach, does this authorise us, asks Angyal, to state that the food before ingestion forms a part of the environment and after ingestion a part of the organism? He also asks whether the egg, when laid, is or is not part of the hen? He concludes that it is impossible to trace a definite line of demarcation between the organism and its environment, since the elements in question are not static structures in space but are opposing directions in the total biological process. The conception of organism and environment as morphological entities which are separable in space is accordingly inadequate for the description of biological phenomena. They become fundamental biological concepts if we define them as dynamic factors. According to Angyal, the biological process of feeding oneself does not begin with the chewing of one's food; the preparation of food, the raising of vegetables, are also "biological" activities in the broader sense of the word and the artificial weapons of

man are just as much so as the natural weapons of the animal (p. 97).

At first sight the biological thesis quoted above finds some confirmation in primitive or child mentality, as can be shown from the writings of anthropologists and ethnologists: savages and infants often claim their excrement as belonging to their bodies.

Etymologically, the term "belong" implies "to go along with," "to be the proper accompaniment," "to form a part." In French, *appartenir* derives from the classical latin *tenere* and comprises a certain number of derivatives (abstain, contain, detain, entertain, maintain, retain, support, wait, relax, extend, etc.). It also implies, however, the idea of *tenir à part*, setting apart; to set an object apart, as if separated, selected in a sense from a mass of objects capable of being possessed in exclusivity. *Tenir* means to subject something, that is to say enslave, submit, subjugate it.

One might perhaps ask whether, in the psychology of personality, there were any real value in adopting such a heterodox viewpoint as that suggested, in this connection, by Angyal, although one must admit that it corresponds with realities that can be observed in the child (aduality), in the primitive, and in those suffering from depersonalisation.

Would it not be better, in this connection, to adhere to the orthodox viewpoint, which distinguishes subject and object, organism and environment, whilst affirming that any separation between them is impossible?

In the study of behaviour, is it not sufficient to distinguish extrinsic and intrinsic relationships of belongingness? "Whatever the mode of integration employed," says Gerard de Montpellier (6), "the elements constituting the synthesis enter into a relationship of belongingness one to the other. But it seems we should distinguish two typical relationships of belongingness: external and internal." The external is based on simple contiguity, that is, on simple concomitance of existence or events, such as the acquisition, for instance, of conditioned reflexes, whereas the internal relationship of belongingness is founded on the structure of the elements or events themselves, in a kind of intrinsic liaison.

Since, according to Angyal, every process which results from the interplay of the autonomy of the organism and the heteronomy of the environment is part of the life process, irrespective of whether it takes place within the body or outside it, and since the term "biological" is not used, in his contribution to the study of personality, as the opposite of cultural, it seems worthwhile to introduce here another observation: the autonomy of the organism includes a relative faculty of choice, which is expressed more particularly in the various forms of integration and identification, but not in the case of inevitable incorporations. It is because of this

inevitability that one says that the most beautiful woman in the world can only give what she has. Facing her mirror, the lovely Cleopatra could do no more than say: "How I would like to change my nose, which they say is too long."

Not to be deprived of the faculty of choice amounts to the power of exercising one's autonomy, of remaining free and independent, being able to give and to receive by relating oneself to others. We distinguish between the man in chains, such as the miser or the fanatic, and the man who is not, who remains free who belongs to himself.

The concept of belonging to oneself is linked with that of the faculty of choice. To belong to oneself is to exercise freely the faculty of thinking, feeling, creating, thus of being free and not conscripted. Autocrats, totalitarian rulers, even the excessively orthodox, deny their subjects or adherents this right, by treating them as traitors, deviationists, heretics liable to punishment for ideological crimes.

Leonardo da Vinci, appearing like a demi-god, that is to say, perfect and separate, said: "When you are alone, you belong entirely to yourself." He feared that personal connections deprive him of the freedom of choice. Thus he glorified solitude and isolation. Nevertheless a tête-à-tête with oneself can also lead to withdrawal to bias, prejudice, passions, complexes, in short to a form of voluntary enslavement.

In the psychology of belongingness, there is another limitation. Plotinus pointed out long ago that man procures what he *can* and preserves what he *has*. The physical environment and psychological surroundings constrain him by imposing their laws. But man only submits as far as it is necessary, and normally he only seeks to dominate within the limits of the possible (3).

If our "mines", if our possessions do not give the exact image of the self, it is because the latter is above all a potentiality, an abstraction (5).

If the self is declared hateful, it becomes less and less so, thanks to the cult of impersonal values.

Whether one calls the organism and the environment two poles, two fields, or simply subject and object, one is obliged in belongingness to distinguish two different elements which are part of a whole.

Instead of studying the interaction of the organism and the environment, Angyal proposes to study life as a unitary whole, which although an individual unit, is still not structureless. It is differentiated along various dimensions. There are two directions: autonomous determination or organismic government, and heteronomous or environmental government. They do not exist independently, but as components of the total biological

process. Two opposite poles, two trends united in the total dynamics of the biosphere, differentiated into two fields, (subject-object).

Although Angyal apologises for having been able to present only a preliminary definition of the concepts, and explains that his terminology, taken from various holistic concepts, is perhaps not always a happy one, and must be considered provisional, his study is an stim contribution to the need for a broad theoretical frame of reference which will permit the integration of the manifold aspects of human nature and behaviour into a unified picture of man. It does not refer exclusively to somatic processes but to the physiologically, psychologically and socially integrated total processes of living.

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THINKING: PSYCHOLOGISTS AND PHYSIOLOGY

BY

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In general psychologists are probably more preoccupied with the scope and limitation of their researches than most other research workers. They have a horror of being vague, and to overcome the vagueness which is inevitable in the early generations of a growing field they have in the main followed two courses. They have constructed relatively elaborate theories on relatively scanty information, and they have emphasised, much more self-consciously than most other scientists, scientific methodology.

Unfortunately the conceptual tools which have so far been developed, and found experimentally useful, are not very helpful in the investigation of thinking, imagination or aesthetic judgment. Yet these processes, and particularly thinking, constitute pre-eminently psychological problems; and many psychologists are disturbed by the apparent inadequacy of existing experimental methods for analysing them. Work on problem-solving and concept-formation seems to deal with only very limited aspects of thought. Solving a problem set in a laboratory and writing a novel are qualitatively so different that the analysis of creative writing in terms of problem-solving appears absurd. At the present time, clinical studies of the impairment of thought seem to yield more insight into the psychology of thinking than experimental investigations. This impression is heightened if one considers psycho-analysis and the work of Piaget (15) as essentially clinical; and certainly neither of these follow strictly experimental procedures.

There is, however, a subtle though important difference between claiming insight into the psychology of thinking and having a theory about thought-processes. A scientific theory, perhaps after an initial guess, must be verifiable. It must have at least some experimental justification and it must be able to furnish predictions about the outcome of future experiments. Insight, on the other hand, provides only the early, intuitive stages of the investigations on which such theories are based. At the most, it expresses an optimistic feeling-tone about the problems, but it remains relatively vague concerning their solution. The dissatisfaction of experimental

psychologists arises from their inability to make their insights sufficiently precise to foster crucial experiments.

PSYCHOLOGISTS AND "INTERNAL EVENTS"

Experiments are nearly always abstractions of processes which in their natural setting appear with many complications. But in the study of thinking this difference between what can be satisfactorily investigated by experiment, and what can not, has had one curious result: the tendency to refer to events which can only be considered as taking place inside the organism. A great variety of such internal events has been postulated, but they fall mainly into two classes. In the first class are those which may be thought proper raw-material for psychological theories in that they are not examined for their physiological characteristics. "Symbolic processes", "Vicarious Trial and Error", and "Insight" are typical members of this class. They are often invoked to account for behaviour in problem-solving and concept-formation. Perhaps also in this class are terms derived from communication-theory, with its distinction between the stimulus and the information that this provides. Since at the most there are only a few useful analogies between communication theory and nervous function, explanatory concepts borrowed from cybernetics cannot be treated as specifically physiological.

The internal events which make up the second class are deliberately physiological. Physiological processes of greater or less complexity have been introduced to provide an element of continuity in otherwise apparently disjointed descriptions of stimulus-response sequences. The physiological character of these events has been admitted, even if regretfully. Hull, (10) for instance, could not avoid using "afferent neural interaction" as a postulate in a psychological theory. Hebb (9) employs intricate nervous networks and phase-sequences in his analysis of complicated psychological processes. In this class also belong the Gestalt theorists' "Isomorphism" (11), Hayek's "Sensory Order" (5) and Head's (6) "Schema". Only a mediation theory such as Osgood (13) elaborates seems to equivocate on whether self-stimulation, for instance, is a physiological or psychological process within the theory.

It may, perhaps be useful at this point to examine the grounds on which the events in each class have been assumed. A recent example of the use of events of the first class appears in Hearnshaw's address to the British Association Meeting of 1954 (7). He speaks of the importance which a theory of thinking must attach to "interiorised patterns of activity", but he carefully avoids any neurophysiological commitments. His internal

events are "representational factors". This, as far as one can gather, can only mean that they are images, though probably in a wider sense than Hume's "copies of impressions". Hearnshaw would not deny that interiorised patterns of activity have physiological as well as psychological characteristics. He probably merely wishes to stress that physiological processes have only other physiological processes as their implication, and that if a correlation between physiological and psychological functions is to be established, the psychological functions must first be determined independently.

For those whose assumed internal events fall into the second class, the study of nervous activity does not merely provide interesting correlations, but also supplies knowledge about the essential links between various stages of psychologically observable phenomena. Thus Hebb (8) believes that in the present state of psychology at least, hypotheses about the growth and history of the phase-sequence can, in principle, account for the simple as well as the more recalcitrant psychological problems of perception, learning and thinking. It can, in fact, achieve a synthesis between these hygienically isolated text-book chapter-headings. A physiological hypothesis may, on this view, give greater unity to psychological theory.

Both schools advocate a policy. The purists maintain that reference to physiology cannot further truly psychological theory; their more pragmatic opponents insist that as a matter of convenience physiological concepts can be useful.

The eclectic may have sympathies, but he would find it hard to arbitrate in this dispute. For a careful examination of the events in either class shows them to be equally indefinite. It is difficult to know quite what process or occurrence is described by "interiorised activity" or "representational factor" or "symbolic processes". But "afferent neutral interaction" and "phase sequences" are also at the best hypothetical mechanisms awaiting confirmation. Indeed, while "phase sequences" are only neuro-physiological possibilities, the "representational factor" (or image) is something with which the psychologist is well acquainted. He even acknowledges it in the laboratory as introspective data, though he may not be sure how to interpret it. The weakness is in the inability to treat the data, not in its absence. And yet the mystique which surrounds physiology is such that many psychologists prefer a physiological guess to an introspective report, which is at least a fact. This preference for physiological hypotheses has its basis in two errors. One is the faulty understanding of the relation between physiology and psychology; the

other is the veneration for objectivity without an adequate analysis of what is meant by "objectivity". These two errors reinforce each other.

PSYCHOLOGY AND PHYSIOLOGY

There can, of course, be no doubt that psychology and physiology are closely connected. But the nature of the connection has often been misunderstood, or wrongly emphasised. Certainly a psychological theory will be inadequate if it implies bodily processes which are from a physiological standpoint inconceivable. This absence of the necessary bodily mechanisms is the fundamental weakness of the Gestalt theory of perception. The brain simply does not function as a homogeneous electrolytic medium (4). But the obligation to be intelligible is mutual. A traditional physiological theory of colour-vision based on three receptors, red, green and blue, must still account for the perception of yellow in peripheral areas which extends beyond the areas in which green and red can be perceived. Psychologists have often been inclined to underestimate the importance of their findings in relation to the discoveries of the physiologists. They have had the general feeling that because the physiologist can point to a muscle or nerve, he is in possession of a "hard fact" for which there is no psychological equivalent. For in contrast to the relative specificity of a nervous impulse as a reaction, the responses of the organism in a psychological experiment seem diffuse and variable.

This difference between physiological and psychological "facts" is further emphasised by the now inevitable use of physiological concepts in psychology. Terms like "stimulus", "response", "inhibition" or "threshold" form essential words in the psychologist's language, and whoever may claim priority of usage, the terms appear to be much more strictly definable in physiology than in psychology. Yet these concepts, when used in psychology, have very different implications from those which they have when they are used in physiology. For instance a physiologically adequate stimulus is by no means necessarily psychologically adequate. The psychological studies of selective perception, subliminal cues, and perceptual defence have their rationale in just these different implications of the same word in the two sciences. It is not simply that psychologically subliminal stimulation predisposes the subject to react "appropriately" to the general stimulus situation. In the subliminal presentation of words, there is a strong suggestion that the associations to such words are *systematically* different from the associations to the same words presented supraliminally (2). Here then, a fundamental psychological difference is the outcome of a difference in degree only of the physical characteristics of the stimulus

which initiate the bodily processes. Again, studies of response dependencies (18) emphasise the absence of any strict and simple relation between the physical characteristics of the stimulus and the subject's responses.

At the most, one can therefore only say that in a sensory system stimulation which exceeds the physiological threshold is a necessary, but not a sufficient condition for exceeding a threshold as found in a psychological experiment. It will be generally admitted that the reactions which are studied in the determination of thresholds, both in neurophysiology and in psychology, are relatively simple types of reaction in comparison to other functions studied in each field. But the disparity between physiological and psychological thresholds implies that a relatively simple function in psychology can only be analysed in terms of complex physiological functions. What is true of thresholds is probably also true of the other words which psychologists and physiologists share. This fact has long been recognised, but its implications have often been ignored and as a result confusions have arisen. These confusions are largely due to a lack of clear thinking about the important subdivision of psychological research which is crudely labelled "Physiological Psychology". There still lingers a suggestion that in "Physiological Psychology" one studies the physiological *basis* of psychological phenomena. This notion is understandable from the standpoint of the history of psychology; but it is logically untenable. It is also misleading. For from this develops the notion that there are two events, a psychological event and a physiological event, with the physiological event somehow *causing* the psychological one. But this is false. Psychologists and physiologists study the same natural events, but from different standpoints. They produce different descriptions, different analyses, of what in an unsophisticated way of speaking is the same total event. The same slow movement of the hand may be described by various observers in very different ways. The physiologist may describe it as a series of subtly organised muscular contractions; the psychologist as a response to a certain stimulus situation; the ballet critic as an expression of exquisite sadness. But these three descriptions would not refer to different events, or to different stages in a causal sequence. To the noble savage there is only one phenomenon: the movement. It is a fallacy to assume that for every different description there is a different event which is described. It is, therefore, equally fallacious to assume that any one event can only be described in one way. In one sense an event is like a playing-card whose significance differs with the rules of different games; in another sense, it is like a cube of which it is impossible to see all sides at once.

Scientists are usually fairly purposive, and so they are careful to assess the usefulness of different types of information to the advance of the particular field in which they are working. Thus, psychologists are interested in the physiological description of events, though they might not be so concerned about their sub-atomic description. It is a little difficult to discover the criteria by which one judges the usefulness, or otherwise, of a scientific rescription of a different type from one's own. Perhaps the most important criterion is based on the manner in which the types of descriptions themselves differ. Such a difference in type is well illustrated by the distinction between psychological and physiological thresholds.

It may now be helpful to use metaphors and to talk of units. Metaphorically, within the language of psychologists "threshold" is a single conceptual unit. The physiological description of what psychologists call a threshold requires a number of conceptual units of physiology, together with statements about the manner in which these are related. Furthermore, some of the physiological units, and some of the types of relation in which they may stand, may appear in more than one physiological description, and for some of these physiological descriptions there may be no single psychological equivalent, or the equivalent may be "threshold". In other words, the conceptual unit of the physiologist has a more general application than those of the psychologist. As a unit it is more general because it is not sufficient to highlight differences between classes of phenomena which the psychologist emphasises by his units. The same situation exists between physiology and, for instance, biochemistry. What is physiologically simple and specific is biochemically complicated, and the units in a biochemical description may appear in more than one physiological context. The more microscopic the unit is, the more widespread is its applicability. However, if one is interested in specific differences rather than in fundamental similarities, the *relation* between the units, not the units themselves, becomes important. But then the biochemical story of a psychological threshold would be so cumbersome that it would be useful to know only in very special circumstances. To ask for it would be like asking for the physical properties of a blend of sherry. The answer could, in principle, be given; it would be a challenge to the physicist to produce it, but it would probably not interest the wine-merchant. Similarly for most practical purposes the psychological threshold does not need an alternative biochemical description. The physiological story is relatively more simple; it has, therefore, some uses which will be elaborated later. For the moment, it is enough to suggest that a scientist is probably most interested in those

"allied fields", whose conceptual units are at the next remove of complexity from his own.

It is important always to remember that the terms which form these "units" are only conceptual. They are aids in talking about the world and the events that occur in it. They are functions which appear in the complicated descriptions which are called scientific theories. The ability to predict on the basis of a theory comes from its descriptive character, for a description is essentially a statement. Predictions, as distinct from guesses, can only be made when one sees the logical implications of a statement. Only concepts and the propositions in which they appear have logical implications. "Events" and "facts" have not. And if the predictions are not fulfilled it is not—in most cases—that the logic is faulty, but because the concepts and the propositions on which they are based are insufficient. Such insufficient concepts must, therefore, be replaced by some which do lead to successful predictions; that is, by concepts which describe the events more adequately.

But while it may in some ways be helpful to consider physiology and psychology as two languages in which events may be described, the analogy may also be misleading. The difference between physiology and psychology as descriptive languages is not like the difference between French and English. In the absence of a dictionary a Frenchman and an Englishman could still learn much of each other's language by pointing; by saying "blanc" and "white" to a sheet of paper, a cloud or a snow-covered field. Their ability to learn from each other comes from their perceiving the world in the same way. On the other hand the essential difference between the physiologist and the psychologist is that, from a scientific standpoint, each perceives the same events differently. It is, perhaps, as if a Martian suddenly appeared in a laboratory and tried to converse with its human occupants. Then gradually it might become clear that the Martian never saw white, but in situations of earthly whiteness beheld instead a rainbow. This would not, however, preclude intelligibility; for though the Martian would never perceive what man perceives, as long as the differences were consistent and systematic, communication would be possible. It would break down only in situations to which either man or Martian, but not both, were insensitive.

The relation between psychology and physiology as languages is more akin to the Man-Martian case than the English-French one. Discovering the relation between psychology and physiology is not finding the physiological causes of psychological phenomena. It is discovering what the physiologist sees, and how the physiologist describes, a given event which

the psychologist also sees and describes. On this interpretation experiments in "physiological psychology" take the place of pointing and exchanging words.

This may seem a curious way of thinking about the work on brain-lesions and learning. Yet it is not unreasonable. The experimental results of such studies are usually summed up in this form; "The rat's ability to learn any of the mazes does not depend on any particular area of the cortex". (Morgan and Stellar, 12, pg. 486). And if this example seems too specifically neuro-anatomical, one from biochemical studies will show these to lead to the same kind of conclusion: "Animals deprived of B-complex vitamins in their first weeks of life later showed marked decrement of learning ability" (Morgan and Stellar, 12, pg. 537).

These findings, and others like them, must be considered at two levels which are very different even though they are related. The psychologist and the physiologist are both interested in learning. The psychologist seeks to find the factors which determine what, how and when an animal learns. The physiologist is interested in the bodily changes which occur during learning, in retention and in forgetting. "Learning" is not itself a proper physiological concept. The psychologist's organism learns; the physiologist's nervous system does not learn, it is functionally and perhaps structurally modified in the course of the general activity which the psychologist classifies as learning. These organic modifications are scarcely understood at the present time. In fact, the psychological investigations of learning are probably much in advance of the physiological analysis of the same events. The contribution of experiments on decortication and learning amount to this: they point to the regions of the nervous system which, when their functions are known more fully, will have as part of their description statements about the possibilities of modification. In other words, when the functions of the nervous system are better understood, they will be elaborated in a general theory; part of such a theory will refer to changes which may take place in the nervous system under certain circumstances. The experiments on cortical ablation simply indicate that, for the rat at least, the physiologist's description of the psychologist's learning behaviour will be part of a general description of cortical function and not part of the description of the function of a particular area of the cortex. The experiments point, in fact, to the subdivision of physiological theories in which the physiological account of learning will be found. They provide a clue to adequate translation.

The analogy with translation may be taken a little further. Just as between French and English there are usually alternative translations for

any one word, so it is clear that there may not necessarily be only one possible physiological and anatomical description which alone can be substituted for a given psychological description, or vice versa. The recovery of function after irreversible neural injury does not leave room for any other conclusion. Since it is assumed throughout this paper that psychology and physiology describe the same events, only differently, it follows necessarily that whatever can be described by one can also be described by the other. In this sense, and in this sense only, is there a logically necessary relation between psychological and physiological propositions. But while it has been generally recognised that physiology can provide an alternative account to the psychological description, it has not been appreciated that the reverse is also possible, even though it is less obvious. Physiological descriptions are in some ways more detailed and subtle, and thus a fine difference between two events which is emphasised in their physiological descriptions may be psychologically too insignificant to warrant attention. This particular difference between physiology and psychology may be more apparent than real; it may be merely a function of the experimental techniques and problems which physiologists and psychologists are interested in at the present time. It is certainly not a logically necessary difference. On the contrary, one can easily conceive of psychologically minute differences which are not physiologically detectable, or which do not interest the physiologist. It is important to distinguish between the ability, in principle, to give alternative descriptions and the usefulness of giving them.

At the theoretical level, therefore, experiments in "physiological psychology" point out where in each theory one will find the alternative account of the same event. There are, however, more immediately practical applications of the findings of such experiments. In some experiments, for instance, in which the normal bodily functions and structures have suffered interference, complex and subtly integrated patterns of behaviour have disintegrated (1). As a result of such disintegration it has become possible to produce a more detailed psychological analysis of these behaviour patterns and their inter-dependence.

There is another class of experiments in "physiological psychology" whose specifically psychological and physiological elements are difficult to determine. These are primarily experiments on the sensory capacity of the animal, though motor and metabolic capacities have also been investigated. It is from experiments in this class that one may get the impression that there is a *causal* dependence of psychological phenomena on *underlying* physiological mechanisms. For instance, it may be said that

human beings cannot hear air-pressure waves of 60,000 c.p.s., because the auditory receptors do not respond to them; or that they cannot see radiant energy having wavelengths of over 1200 μ , because the visual receptors are insensitive to them. This kind of statement gives the right information in a misleading way.

In the first place, it is important to note that "seeing" or "hearing" are psychological and not physiological terms. Moreover animals do not see wavelengths and do not hear frequencies. They see colours and they hear tones. Wavelengths and frequencies are physical characteristics of the stimulus and not qualities perceived by the organism.

Secondly, if, for example, "seeing" is the psychological description of the event, then the physiological alternative is a series of statements about certain nervous structures and their activity. The connection between seeing and the activity of the visual mechanism arises from the fact that whenever the subject says "I see X", the physiologist can observe (some of) the activities in certain parts of the nervous system. Indeed the classification of these parts and their function as "visual mechanisms" is only justified because they are active when the subject says "I see", and because the subject can no longer see when they are damaged. The bodily changes which the physiologist describes do not, at the *end* of their activities produce a psychological process called "seeing". The psychologist does not describe something which is a final product. He furnishes an alternative description with its own limitations and advantages. And descriptions do not cause or produce each other. They do not even strictly entail each other. They merely apply, as a matter of fact, to the same event.

It would perhaps be best to consider statements about sensory capacity as essentially physiological rather than psychological statements. The experimental procedures for studying the senses combine psychological and physiological techniques; but the theories to which such experiments give rise are mainly about bodily functions as the physiologist describes them. Moreover, if one thinks of such studies as psychological there will be a temptation to treat psychology as a branch of physiology. And with that attitude goes a most insidious belief that when physiologists know more, all psychological statements will be reduced to physiological laws. But to say that psychology is a branch of physiology (3, pg 4), a branch which deals with the physiology of the intact organism, is like saying that civil engineering is a branch of physics; the physics of bridgebuilding. At the level of the analysis of events at which this is true, it is very uninformative. It is as useless as saying that neurophysiology is the biochemistry and biophysics of nervous activity. At the most, it indicates

what alternative descriptions of an event are possible. It ignores the fact that different descriptions are important because they emphasise different aspects of an event, and that they are convenient because one description may summarise in a single concept features of an event which in another description would need a long and cumbersome elaboration.

From a psychological standpoint the physiologists' descriptions are, in a sense, descriptions of part of the environment. For psychological purposes the difference between the external and the internal environment has generally received wrong emphasis. This is partly due to the fact that, physiologically, the concept of "internal environment" refers, roughly, to the fluids which surround the more stable anatomical structures of the body. But for psychologists such a limited interpretation of "internal environment" is unwarranted; as unwarranted as limiting the term "external environment" to the atmosphere which immediately surrounds the body and through whose medium stimuli must pass to reach the organism. For psychology there is an essential continuity between external and internal environments. Alterations in either may affect the behaviour which the psychologist studies. Perhaps one of the most important theoretical suggestions of Gestalt psychology lies in its stress of this continuity. Gestalt theory breaks down because it sees the continuity as a continuity of physical systems and underestimates the complexity of the body considered as a physical system. For experimental psychology bodily structures are part of the environment. Physiological descriptions are descriptions of that environment; they do not, however, answer psychological questions asked in the search for better psychological descriptions.

It follows from this argument that reference to "internal events" of a physiological order does not further the psychological analysis of thinking. If Hebb, (8, 9) and others like him, find physiological concepts helpful in their study of thinking, this is an indication of the way in which they themselves think; it is not due to any necessary connection between specific forms of neural activity (real or imagined) and the psychological account of thought-processes. Brain-lesions are psychologically important because they may prevent "normal" thought-processes; not because they show which part of the brain, with what types of anatomic organisation, are necessary for thinking.

PSYCHOLOGISTS AND "OBJECTIVITY"

But the misconception concerning the connection between physiology and psychology is not the only reason for the apparent plausibility of using physiological notions in psychological explanations. Another reason

is the belief that the physiologists' findings are in some manner more objective than the psychologists'. For nerves can be seen and muscles felt. This concept of "objectivity" needs to be examined.

It is not easy to find what it is of which "objective" is a suitable predicate. During ordinary conversations it is generally used in value-judgments. One calls an account, a report, or an arbitration in a dispute "objective", meaning that it is free from personal prejudice. This kind of objectivity should be common to all research, whether in the Sciences or in the Arts. Lack of this kind of objectivity is also common to all academic studies; it shows itself usually in an almost emotional insistence on a particular theoretical framework for organising the facts. But it is not the failure to be objective in this sense that worries the psychologist.

The objectivity which the psychologist seeks can only be defined in terms of its presumed opposite "subjectivity". The phenomena which the psychologist—and others—describe as "subjective" are *experiences*. An experience, qua experience, can only happen to the individual who has it. Even the most subtly self-aware person, with the greatest powers of description cannot communicate his experience to others. At the most he can only evoke in them experiences similar to his own. But these will then be *their* experiences and not *his*. An experience is, therefore, an ineffable, deeply personal occurrence which can never be known by anyone else.

On the other hand, science can only concern itself with what can be known by all. Yet psychology, at least in its origin, is precisely the study of feeling, thinking and consciousness, memory and emotion. And these are essentially personal and subjective experiences. Others may see the expression of feeling, the product of thought and the powerful effects of emotion. But the feelings, thoughts and emotions themselves must always remain private and secluded from general inspection. They must always remain internal events known only to the individual himself—and to him perhaps only in part.

It therefore seems that psychology cannot be at once a science and provide an adequate account of such behaviour as thinking. Only stimulus situations and the responses associated with them, can be observed by all; and, therefore, they alone can form the raw-material for scientific psychology. Of course, sometimes one must allow that processes take place inside the organism which intervene between the stimulus and the response. But these can either be inferred and measured in terms of their effect on the stimulus-response relations, or else they can only be considered scientifically in their physiological form. Reports by the subject on how he solved a problem or learned a list of nonsense syllables do not come

under this category. For not to be sure that the factors isolated are in fact, the crucial factors of thought, such reports tell us which he had perhaps not noticed. These are reports of essentially internal processes assumed that they can never be observed. The principle of oscillation or a

While it is true that science is derived from observations, it is false to regard it as scientific untouchables. The importance has been under-

Historically, introspection has been central in its philosophic form. But the decline of introspection was under-standable. It depended on the belief in the possibility of knowledge by careful examination of his own mind. By the time of Freud, the standing of his nature. By the time of Freud, he succeeded in showing the complexity of the mind. He had emphasised the influence of unconscious behaviour; and Freud was based on the irrationality rather than on rationality. His work on himself had not produced the expected results. It was ways ironic that Freud, who had been a pioneer of psychoanalysis, should have helped to bring about the decline of introspection. This decline was due to the considerable advances of physiology and the interests between physiology and psychology. The source of psychological experience is

In addition to this historical background to the decline of introspection, the experience cannot be denied to be exaggerated. It is true that one cannot have another's experience just as he cannot have another's sense of ownership—are part of the same. But the perceptions of stimuli are private. Admittedly the subject is not aware of the stimuli without knowing about the stimuli. The introspective reports about so-

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. For nobody, not even the most careful experimenter, can isolate factors as important as those determining his behavior. Experiments tell the experimenter a great deal, but they are not noticed. But fundamentally incommunicable experiences can never be as objective as an observation or a hypothetical phase. That science deals only with observable is false to class the introspective variables. There are, however, many that are underestimated.

Introspection provided the basis for behaviorism. But already in the nineteenth century it was undermined. The original belief in the rationality of man, in his ability to know of himself, he would come to realize. By the end of the nineteenth century the continuity of man was being questioned. The influence of man's economic environment was beginning to elaborate the idea of the reasonableness of man. It produced a very tasteful and useful attitude, who depended so much on the decline of man in his own hands. Advances of physiology and psychology and psychology is the basis of psychology and the acceptance of a biological explanation.

This historical background the decline of introspection. It can be denied, but it can be, and it is true that one man cannot have another's toes. The implications are part of the meaning of stimuli in a psychophysical experiment. The subject may make mistakes about them. He may equalize about solving a problem. But

under this category. For nobody, not even the subject himself, can really be sure that the factors isolated as important by him in his report were, in fact, the crucial factors determining his behaviour. At the most, it is thought, such reports tell the experimenter about environmental cues which he had perhaps not noticed. But fundamentally introspective reports are reports of essentially incommunicable experiences. It is, therefore, assumed that they can never be as objective as an inferred and interpolated principle of oscillation or a hypothetical phase-sequence.

While it is true that science deals only with observations and inferences from observations, it is false to class the introspective reports of experiences as scientific untouchables. There are, however, several reasons why their importance has been underestimated.

Historically, introspection provided the basic materials of psychology in its philosophic form. But already in the nineteenth century the status of introspection was undermined. The original esteem of introspection depended on the belief in the rationality of man, and on the belief that by careful examination of himself, he would come to a complete understanding of his nature. By the end of the nineteenth century, Darwin had succeeded in showing the continuity of man with other animals; Marx had emphasised the influence man's economic environment had on his behaviour; and Freud was beginning to elaborate a theory which stressed the irrationality rather than the reasonableness of human conduct. Man on himself had not produced a very tasteful autobiography. It is in many ways ironic that Freud, who depended so much on introspective information, should have helped to form the attitude which decries the value of introspection. This decline of man in his own eyes, taken together with the considerable advances of physiology and the genuine continuity of interests between physiology and psychology is the basis of the rejection of introspective psychology and the acceptance of physiology as the source of psychological explanation.

In addition to this historical background there is also a philosophic background to the decline of introspection. The subjective nature of experience cannot be denied, but it can be, and has persistently been, exaggerated. It is true that one man cannot have another's experiences—just as he cannot have another's toes. The implication of privacy—and a sense of ownership—are part of the meaning of the word "experience". But the perceptions of stimuli in a psychophysical experiment are no less private. Admittedly the subject may make mistakes in these, mostly without knowing about them. He may equally make mistakes in his introspective reports about solving a problem. But because one sometimes

makes mistakes, because one sometimes has illusions, it does not follow that one always makes them, or even that one makes them frequently. Indeed, if it were not perfectly reasonable to assume the general correctness of reports of sensations and experiences, one could not use the words "illusion" or "dream" as fruitfully as one does. Only the solipsist needs to rely on the subjective nature of experience as a basis for his argument. There is nothing subjective about the introspective report as a report; if its tendency to variation makes it awkward to treat quantitatively, that is a problem of measurement, not of objectivity. Such variations are errors of measurement inherent in the very act of measuring. And even physicists cannot free themselves from this handicap. When Pavlov (10) (and after him Watson, 17) insisted that he was replacing introspective psychology by an objective psychology, he was propounding arguments whose polemic value at the time was most important. Introspection had failed to become quantitative. Unfortunately methodologists have accepted this state of affairs as inevitable rather than as a challenge. But their main function is precisely to find new techniques for recalcitrant problems; instead they have concentrated on refining procedures for investigating what is known already. They are sticklers for method rather than grapplers with problems; and in this they are the true heirs of what was worst in mediæval scholasticism.

CONCLUSION

The fall of man in his own estimation; the faulty emphasis on the subjective aspects of experience—as distinct from those of its features which can be shared—and the barrenness of self-conscious methodology on important issues, have all led to a dismissal of introspection. Yet the feeling that internal events are necessary to give continuity to the psychological account of behaviour has remained. It has found an outlet in the introduction of physiological concepts or the postulation of unfortunately equally vague "representational factors" of a psychological variety. Both are unsatisfactory. The use of physiological hypotheses as such cannot answer psychological problems; it is simply a form of intellectual displacement activity. On the other hand the term "representational factors" only suggests a new class-name for various types of images and symbols. It does not itself indicate any ways of investigating them.

There are only two possibilities. Psychologists can abandon all pretence of including internal events in their theories. This would almost certainly mean abandoning the investigation of thought-processes qua thought-

processes; instead successful and unsuccessful thinking and the difficulties of solving a problem might be analysed in terms of the "stimulus" difficulty of the problem and its "phenomenal" characteristics (19). However, this procedure ignores the processes which occur in arriving at the solution; processes which are at least partly capable of being recorded as introspective reports. The images and symbols, which appear in introspection, are purely psychological phenomena, similar in kind, though differing in detail, to perceptual phenomena. They need to be related to physiological functions, but they are not only physiological functions. This is clear from the fact that they *are* introspective whereas a nerve impulse as such is not. Consequently, the psychological problem is not as Hebb (9) seems to suggest, how introspection is possible in terms of neural organisation. Introspection is a psychological phenomenon in its own right. It may not give a very clear picture of thinking considered solely as reasoning, but perhaps the identification of thinking with syllogistic reasoning is merely an heirloom handed down from defiantly rationalist days. It may certainly be fruitful to consider that introspection provides a true, even if partial reflection of the "internal" aspects of thinking. The objection that there is no necessary connection between such reports and "what really happened when the subject thought" is not as serious as it seems. There are no grounds to suppose that nerve impulses are in some way more "real" than the images which the subject reports. The only connections which are *necessary* are logical connections. But in the investigation of thinking one is not concerned with the *logically* necessary connection between introspective reports and thought-processes; one is concerned with whether these reports *as a matter of fact* can be used to discover how people think. The search for what thought really is, whether it is images, or words, or nerve impulses, etc., is a delusion. Thought is *all* that anyone can find out about it, psychologically, physiologically, biochemically or in terms of whatever level of sub-atomic physics is fashionable. The dainty reluctance with which introspective reports have been accepted as "additional" information, is unwarranted. Introspection is no more or less indirect or an artefact than the stained slides of the anatomist, or the oscillograph records of the physiologist, or the readings on the dials of an atomic reactor are indirect or artefacts.

SUMMARY

Contemporary experimental psychology seems to have neither the techniques nor the conceptual tools which are needed to investigate adequately the traditional problems of psychology associated with thinking. This has tempted psychologists to look to physiology for an "objective" analysis of thought. But such physiologising is based on a misconception of the relation of physiology to psychology. This paper attempts to examine that relation. It also suggests that psychologists are unduly perturbed about the "objectivity" of their findings, because they have often failed to consider what the word "objective" means.

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COMMUNICATION OF THE INTERNATIONAL UNION OF SCIENTIFIC PSYCHOLOGY

The Council of International Organizations of the Medical Sciences met on September 30 and October 1, 1955. Henri Piéron has sent in the following account of the nature of the Council, and of this particular meeting.

REPORT ON THE THIRD ASSEMBLY OF THE COUNCIL OF INTERNATIONAL ORGANIZATIONS OF THE MEDICAL SCIENCES

BY

HENRI PIÉRON

On the invitation of the Executive Secretary, Dr. Delafresnaye, addressed to the International Union of Scientific Psychology, I represented the Union as an observer at the Third Assembly of the Council, held at UNESCO House in Paris on September 30 and October 1, 1955.

I had received all the working papers sent to the participants and I was able to follow the discussions, so as to obtain precise knowledge of the organization of the Council and to prepare a report for the information of the Union.

THE CREATION AND DEVELOPMENT OF THE COUNCIL

In April, 1949, there was held in Brussels the constitutive assembly of a permanent Council for the Coordination of International Congresses of Medical Sciences (CCICMS). Professor Fleming had advised our Secretary General to arrange for the participation of our Union in the creation of this Council, rather than to request admission into ICSU, of which he was then President. I was of the opinion, however, that scientific psychology could not be completely integrated with the medical sciences.

The Council was created by 44 international organizations which truly became participant members. Statutes were adopted, and financial aid was voted by UNESCO and the World Health Organization; the Council was thus enabled to give subventions to a certain number of congresses and symposia, to attempt the coordination of congresses having related goals, to finance lectures which represented a sort of "advanced course" on the occasion of congresses, and to contribute to the travelling expenses of distinguished participants.

At Geneva, in April, 1952, there was a Second General Assembly, which modified the name of the Council (now CIOMS), amended the statutes, retained the Executive Committee of which Professor Maisin of Belgium was chairman, and elected Professor Visscher, American physiologist, President for a three-year period.

The statutes now in force indicate that regular membership in the Council is open to non-governmental organizations which are fully international, engaged in activity of a medical character, that is to say, devoted entirely or mainly, whether in the field of theory or application, to (a) improvement of health, (b) the encouragement of scientific research on the principles to be followed for the maintenance of health as well as on the causes of disease, (c) the study of methods of prevention

and of treatment (Article 4). Those international non-governmental organizations whose disciplines do not satisfy these requirements may become part of the Association under the classification of adhering members (Article 7). Any requests addressed to the Executive Committee, and supported by two member organizations of the Council, are submitted to the General Assembly and decided on the basis of a two-thirds majority of those present and voting (Article 6). Each member organization has one vote, but a representative cannot have more than one vote even if he represents several organizations (Article 17).

THE THIRD ASSEMBLY

The meeting was under the presidency of Prof. Visscher, assisted by Prof. Maisin, chairman of the Executive Committee, and by the Executive Secretary. There were addresses of welcome and good wishes by representatives of UNESCO and WHO. The applications of 4 organizations as regular members, and of 3 others as adhering members, were unanimously approved, without discussion.

The Associations whose term had expired on the Executive Committee (which includes 12 members elected for nine years) were reelected.

The report of the Chairman of the Executive Committee and the financial report were adopted. There was discussion of questions relating to Congresses and symposia; the latter appeared to be regarded as the most fruitful variety of international meetings.

Many organizations had been invited to send observers, including ICSU, the International Council of the Social Sciences, the International Council of Philosophy, the International Society of Normal and Pathological Ethnopsychology, the International Union of Biochemistry, the Permanent Committee of International Congresses of Genetics, etc.

Among the members of the Council and their representatives might be mentioned the International Group for the Coordination of Psychiatry and Psychological Methods (Dr. Germain), the World Federation for Mental Health (Dr. Por'Her), the World Congresses of Psychiatry (Dr. Sivadon), the International Society of Criminology, the International Union of Physiological Sciences (which this year was admitted into ICSU), the International Association of Gerontology, the International Conference of Audiology.

SOME DATA FROM THE REPORTS

Between the years 1952 and 1954, three meetings were held in connection with the coordination of Congresses: one organized in 1953 by President Visscher in Minneapolis dealt with the 7th International Conference of Social Work, the 14th International Congress of Psychology, the 19th International Congress of Physiology, the World Federation for Mental Health, etc. It appears that no coordination whatever resulted.

The coordination of the Congress of Physiology with that of Electroencephalography was apparently arranged independently of CIOMS, but the Council did organize in Canada, in association with these Congresses, a symposium on "Cerebral Mechanisms and States of Consciousness", presided over by Prof. Jasper with 20 participants (including 9 Europeans, who received from the Council a financial contribution corresponding to one-third of the round-trip ticket).

On the occasion of the Psychological Congress in Stockholm in 1951, a meeting

of the Group for the Coordination of Psychiatry and Psychological Methods received support from the Council.

The extremely useful work of the Council can be realized only in terms of its budgetary possibilities. In this connection, although such possibilities in the first years, without being great still proved to be effective, thanks to UNESCO and WHO, they are rapidly being reduced, and the outlook for the future is discouraging. In 1950 and 1951, CIOMS had a budget of a little more than \$60,000 per year. For the three years 1952-1954, the subventions totalled \$147,700 (\$89,200 from WHO and \$55,500 from UNESCO) and membership dues (varying from \$20. to \$100.) amounted to \$13,113. That means for each year an average of \$53,600, but actually more in the first year, and less and less in each succeeding year.

The representatives of UNESCO and of WHO both announced a reduction in their subventions (in the latter case from \$25,000 to \$5,000), explaining that their aid was meant to facilitate the formation of a new organization, which should then become self-sufficient. As a result the prospects are poor (since administrative expenses have represented a quarter of the total resources).

Specific subventions from the Council include, e.g. \$3,000 to the 2nd International Congress of Biochemistry (which sends observers but is not an adhering member); \$5,000 to the 19th Congress of Physiology; \$2,000 to the 3rd Congress of Electroencephalography in 1953; \$2,500 to the 5th International Congress of Neurology; \$2,000 to the 3rd International Congress of Gerontology; \$2,000 to the 5th International Congress on Mental Health, in 1954; \$300 to the 3rd International Congress of Criminology in 1955.

There can be no doubt that the financial problem is the most serious one for the future of CIOMS.

INTERNATIONAL COLLOQUIUM ON FACTOR ANALYSIS

BY

M. REUCHLIN

An International Colloquium on *Factor Analysis and its Applications* was organized by the National Centre for Scientific Research (Centre National de la Recherche Scientifique) with the aid of the Rockefeller Foundation, and met in Paris from July 11 to 16, 1955.

All the participants regretted the absence of Burt and Thurstone, both of whom had intended to be present, and had sent in communications. They were both unfortunately prevented by ill health from participating.

Since the papers were all distributed to the participants several weeks in advance of the Colloquium, most of the time of the meeting could be devoted to discussion.

After the introductory speeches by the Director of the Centre National de la Recherche Scientifique, and by Prof. Laugier of the Sorbonne, whose initiative was responsible for the Colloquium, and a report by Mr. Peel on the results of the Upsala symposium in 1953, the group turned to a discussion of the individual communications, each of which was defended by its author. The only exception to this rule were the papers by Thurstone on "Current Problems and New Methods in Factor Analysis" and by Burt on "Factor Analysis: Methods and Results". They

did, however, serve to stimulate an exchange of views among the participants. Discussion then turned to the following papers, in the order of their presentation:

- GUILFORD, J. P. — Dimensions of intellect.
- PIÉRON, H. — Le problème général de la recherche et de la nature des "facteurs" en psycho-physiologie.
- HOTELLING, H. — Relations of the newer multivariate statistical methods to factor analysis.
- PINEAU, H. — Remarques sur l'analyse factorielle de Hotelling et comparaison avec les méthodes centroïdes.
- BARGMANN, R. — A Statistical test for the stability of simple structure.
- YELA, M. — Psychological meaning of factor analysis as a research method.
- BERNYER, G. — Les facteurs psychologiques: quelques remarques sur leur nombre, leur identification, leur nature.
- REUCHLIN, M. — Facteurs observés et facteurs théoriques en psychologie.
- GUTTMAN, L. — The Radex approach to factor analysis.
- FAVERGE, J. M. — Utilisation du schéma de Spearman dans le calcul des images.
- EYSENCK, H. J. — Factor analysis and the problem of validity.
- SCHREIDER, E. — Emploi de l'analyse factorielle dans l'étude de la variabilité biologique.
- LEDERMANN, S. — Application de l'analyse factorielle à l'étude factorielle de la mortalité.
- DARMOIS, G. — Observations théoriques sur l'analyse factorielle linéaire et générale.
- DELAPORTE, P. — Nouvelle méthode de statistique mathématique pour l'estimation des facteurs et de leur écart-type en analyse factorielle.
- EL KOUSSY, A. H. — Trends of research in space abilities.
- HUSEN, T. et HENRYSSON, S. — Factor analysis of achievement tests.
- PEEL, E. A. — The factor analysis of person correlations and the use of independent determiners to identify the factors.

In Burt's absence, Reuchlin presented a general synthesis at the end of the discussions.

All the communications and the discussions (each discussant has been asked to write out his comments) will be published in French, in one volume, by the Centre National de la Recherche Scientifique, 13 Quai Anatole-France, Paris VII, probably early in 1956.

INTERNATIONAL ASSOCIATION OF APPLIED PSYCHOLOGY

The twelfth Congress of this Association was held in London from the 18th to 23rd July 1955, when there was an attendance of 550 delegates. Meetings of the Congress were organised partly as symposia and partly as groups of individual papers related to a particular theme. There were four plenary sessions with symposia on subjects of interest to all in the field of applied psychology, and fifteen other symposia, each devoted to some special theme. In addition there were forty individual papers. There were three evening lectures by Sir Frederic Bartlett, F.R.S., Professor E. Mira y Lopez and Professor C. Pellizzi.

The Organising Committee of the London Congress are proposing to publish a volume of Proceedings which will contain papers in full presented at the Major Symposia, the Opening Session and the three Evening Lectures. Summaries of the other papers will be included. The complete text of some of these papers will appear in one or other of the scientific journals, while the European Productivity Agency will be publishing twenty-one papers which are concerned with industrial psychology.

At the General Meeting of the Association held during the Congress, it was decided to change the title of the Association to: "Association Internationale de Psychologie Appliquée—International Association of Applied Psychology". The Officers of the Association were re-elected as follows:—

President:	Dr. C. B. Frisby (London)
Past President:	Prof. H. Piéron (Paris)
Vice President:	Prof. J. Germain (Madrid)
General Secretary:	Prof. R. Bonnardel (Paris)
Treasurer:	Dr. M. Coumétou (Paris)

The next Congress will be held in Madrid in 1958 under the Presidency of Professor José Germain. Further details about the London Congress and on the organisation of the next Congress in Madrid will be given in forthcoming issues of the Bulletin of the Association.